THE ACCOUNTING REVIEW
Vol. 97, No. 4
July 2022
pp. 287–316

American Accounting Association
DOI: 10.2308/TAR-2019-0266

Can FinTech Competition Improve Sell-Side Research Quality?

Russell Jame
University of Kentucky

Stanimir Markov
The University of Texas at Dallas

Michael C. Wolfe
Oklahoma State University

ABSTRACT: We examine how increased competition stemming from an innovation in financial technology influences sell-side analyst research quality. We find that firms added to Estimize, an open platform that crowdsources short-term earnings forecasts, experience a pervasive and substantial reduction in consensus bias and a limited increase in consensus accuracy relative to matched control firms. Long-term forecasts and investment recommendations remain similarly biased, alleviating the concern that the documented reduction in bias is a response to broad economic forces. At the individual analyst level, we find that bias reduction is more pronounced among close-to-management analysts, and that more biased analysts respond by reducing their coverage of Estimize firms. The collective evidence suggests that competition from Estimize improves sell-side research quality by discouraging strategic bias.

Keywords: analysts; forecast bias; FinTech; crowdsourcing.

The bigger effect from the fintech revolution will be to force flabby incumbents to cut costs and improve the quality of their service. That will change finance as profoundly as any regulator has.
—*The Economist* (2015, 14)

I. INTRODUCTION

The financial services industry plays an important role in the economy, but it is prone to conflicts of interest (Mehran and Stulz 2007) and is inefficient and ripe for disruption (Philippon 2016). The loss of confidence that ensued after the global financial crisis of 2008 spurred a wide range of financial technology (FinTech)-enabled business model innovations, with young start-ups and established technology firms leading the way. Accordingly, a small, but fast-growing,

We thank Xiumin Martin (editor), two anonymous referees, Iavor Bojinov, Andy Call, Michael Chin, Pouyan Ghazizadeh, Clifton Green, Jillian Grennan, Stephan Hollader, Patrick Hopkins, Xing Huang, Julian Kolev, Roby Lehavy, Mario Schabus, Rick Sen, Christoph Sextroh, David Veenman, and seminar participants at Arizona State University, Baruch College–CUNY, Florida State University, Oklahoma State University, Rutgers, The State University of New Jersey, Southern Methodist University, Tilburg University, University of Amsterdam, University of Illinois at Chicago, University of Kentucky, UNSW Sydney, The University of North Carolina at Charlotte, University of South Florida, University of Technology Sydney, West Virginia University, the 2017 AAA Annual Meeting, 2019 AFA Annual Meeting, 2017 Colorado Summer Accounting Conference, 2017 Catolica|Nova Lisbon Accounting Conference, and 2017 Financial Intermediation Research Society Conference for helpful comments, and Leigh Drogen and Josh Dulberger from Estimize for providing the data and answering our questions.

Russell Jame, University of Kentucky, Gatton College of Business and Economics, Von Allman School of Accountancy, Lexington, KY, USA; Stanimir Markov, The University of Texas at Dallas, Naveen Jindal School of Management, Department of Accounting, Richardson TX, USA; Michael C. Wolfe, Oklahoma State University, Spears School of Business, School of Accounting, Stillwater, OK, USA.

Editor's note: Accepted by Xiumin Martin, under the Senior Editorship of Mary E. Barth.

Submitted: May 2019
Accepted: September 2021
Published Online: September 2021

literature studies how FinTech competition affects the incumbents in order to depict and understand the emerging equilibrium, conduct welfare analysis, and inform regulators and policy makers.[1]

In this study, we explore whether FinTech competition can improve the quality of sell-side analyst research. Sell-side analysts are a primary investment research provider in capital markets, whose incentives to be accurate and unbiased often conflict with their incentives to please corporate managers and generate brokerage and investment banking business (Irvine 2004; Jackson 2005; Lin and McNichols 1998; Michaely and Womack 1999). These conflicts of interest can result in substantial costs in the form of market mispricing (Dechow and Sloan 1997; Veenman and Verwijmeren 2018) and wealth transfers from less sophisticated to more sophisticated investors (Malmendier and Shanthikumar 2007; De Franco, Lu, and Vasvari 2007).[2] As evidence that FinTech-provided research conveys new information to capital markets quickly accumulates (Chen, De, Hu, and Hwang 2014; Jame, Johnston, Markov, and Wolfe 2016; Avery, Chevalier, and Zeckhauser 2016; Bartov, Faurel, and Mohanram 2018), the question of whether and how FinTech competition impacts sell-side research has assumed new importance.

We focus on a popular FinTech provider of crowdsourced earnings forecasts, Estimize, because it has distinctive features that make it especially well-suited for testing the FinTech competition hypothesis. First, Estimize forecasts are not only widely distributed through data feeds and financial platforms (e.g., Bloomberg) and regularly referenced in the financial press, but they are also close to unbiased, reasonably accurate, and incrementally informative (Jame et al. 2016), suggesting they are a valuable source of new information in capital markets and a viable sell-side competitor. Second, since Estimize focuses almost exclusively on short-term earnings forecasts, we can use short-term sell-side forecasts to test whether FinTech competition affects sell-side research quality and use long-term sell-side forecasts and stock recommendations in placebo tests to alleviate the concern that the arrival of Estimize coincides with a major market event. Finally, the availability of plentiful data and well-defined measures of quality, such as forecast bias and accuracy, make earnings forecasts an excellent laboratory for studying changes in incumbent behavior.

We test whether FinTech competition improves sell-side research quality using a difference-in-differences (diff-in-diff) approach. We posit that sell-side analysts covering a particular firm experience an increase in FinTech competition in the year the firm is added to the Estimize platform and crowdsourced forecasts become available. We construct the treatment sample as (1) firms added to Estimize in 2012 (*First-Year Treatment*), or (2) firms added to Estimize in the period 2012–2014 (*Staggered Treatment*).[3] For each treated firm, we select a matched control firm using a propensity score model that includes size, book-to-market, sell-side coverage, turnover, and the bias and accuracy of short-term forecasts. We define quality as forecast unbiasedness and accuracy.

We find large declines in consensus forecast bias in both the *First-Year Treatment* and *Staggered Treatment* samples. The diff-in-diff estimates, multiplied by the respective sample standard deviation of consensus bias, imply a greater than 80 percent decline in forecast pessimism from the pre-Estimize level. Importantly, analysis of consensus bias in the pre-Estimize period yields no evidence of pre-trends. In contrast, consensus accuracy does not increase in either sample. These findings are robust to variation in event window length, alternative matching approaches, alternative measures of bias, accuracy, and consensus, and excluding firms with guidance. Our results on bias reduction are further strengthened by two placebo tests. Specifically, we do not find a decline in statistical forecast bias, alleviating the concern that Estimize coverage is correlated with positive performance shocks. More importantly, we do not find a change in bias for longer-horizon forecasts and stock recommendations, alleviating the concern that Estimize coverage is correlated with broad unobservable forces that steer sell-side analysts toward unbiasedness.

Consistent with theoretical models that predict that heightened competition increases incentives for reputational building and reduces conflicts of interest (e.g., Horner 2002; Gentzkow and Shapiro 2008), we find that research quality increases more when Estimize competition is more intense. Specifically, the decline in consensus bias approximately doubles when the Estimize consensus is historically less biased, or when Estimize industry coverage is above the sample median. In addition, we find a statistically significant increase in accuracy when competition is sufficiently intense: i.e., the Estimize consensus is historically less biased and more accurate, and Estimize industry coverage is above the sample median. The robust and pervasive decline in bias and the limited accuracy improvement suggest that the primary effect of Estimize is to discourage

[1] See a recent survey by Chemmanur, Imerman, Rajaiya, and Yu (2020). Following Chemmanur et al. (2020), we define FinTech firms as newly founded firms that use financial technology to offer more innovative solutions to problems in traditional banking, insurance, and asset management areas, and acknowledge that incumbents can also use financial technology to improve the quality of existing services and provide new ones.

[2] We acknowledge that not all studies find evidence of analyst research being biased and detrimental to market efficiency and investor welfare (see a survey by Mehran and Stulz [2007]).

[3] We note that the benefit of increasing the sample size and staggering the treatment may be outweighed by the cost of weakening treatment intensity: firms added after 2012 attract fewer Estimize contributors (less than 2.50, on average) compared to those added in 2012 (11.43).

strategic bias rather than to encourage effort.[4] Because Estimize forecasts are much less biased, but similarly accurate (Jame et al. 2016), they are perhaps more effective in revealing analyst bias than analyst inaccuracy.

Next, we analyze individual analyst forecast data to better understand the sources of improvement in consensus forecast quality, as well as the mechanisms through which competition operates. Consistent with the hypothesis that Estimize discourages strategic bias, we observe a greater decline in relative forecast bias (relative to other analysts in the same firm-quarter) among analysts close to management, as proxied by the existence of an underwriting relationship or the tendency to issue more favorable recommendations. We find no evidence of a relation between analyst forecast quality and Estimize forecast availability at the time of forecast issuance, which suggests that the value of Estimize forecasts as a learning resource to analysts is, at best, limited. Finally, we observe that poor performance, defined as issuing forecasts in either the bottom decile of accuracy or the top decile of bias in the prior quarter, is less persistent in the post-Estimize period, consistent with Estimize heightening competitive pressures on poor forecasters.

Analysts exposed by Estimize as providing low-quality research may respond by improving research quality (intensive margin effect) or by avoiding direct competition from Estimize (extensive margin effect). Both responses are borne out by the data. Specifically, we find that our bias reduction results hold at the individual forecast level after including analyst-firm fixed effects. In addition, analysts in the top decile of past relative bias are more likely to shift coverage away from Estimize stocks in favor of non-Estimize stocks. We do not, however, find that analysts in the bottom accuracy decile behave differently from other analysts, echoing our earlier finding that Estimize is more effective in eliciting changes in bias than changes in accuracy.

Our primary contribution is to paint a more complete picture of how FinTech is changing the process by which information is produced and revealed in capital markets. Our findings suggest that FinTech is not only creating new sources of value-relevant information and leveling the informational field between institutional and retail investors (Chen et al. 2014; Jame et al. 2016; Farrell, Green, Jame, and Markov 2022; Gomez, Heflin, Moon, and Warren 2020), but also impelling the incumbent providers, sell-side analysts, to produce higher-quality research (this study). More broadly, our study illustrates how a technological innovation that allows individual investors to produce and disseminate earnings estimates can disrupt the traditional Wall Street information ecosystem (Costa 2010). In doing so, our study complements concurrent work that examines the disruptive use of technology to mass-produce recommendations (Coleman, Merkley, and Pacelli 2021) and, more generally, to alter the market for financial analysis (Grennan and Michaely 2020).

Our study also adds to the literature that explores the forces constraining analyst conflict of interests, which include reputational considerations (e.g., Fang and Yasuda 2009), regulation (e.g., Barber, Lehavy, McNichols, and Trueman 2006; Kadan, Madureira, Wang, and Zach 2009), and analyst competition (Hong and Kacperczyk 2010; Merkley, Michaely, and Pacelli 2017). Our study most closely relates to prior studies on the moderating role of competition, but differs from them in one major respect. Specifically, while prior work focuses on the decrease in *internal* competition (i.e., competition from other sell-side analysts) due to analyst departures (e.g., Hong and Kacperczyk 2010; Merkley et al. 2017),[5] we explore a potential increase in competition due to the emergence of Estimize: an *external* competitor whose business model and practices differ dramatically from those of the incumbents. Our findings that analyst bias declines when Estimize enters the market for earnings estimates directly complement and extend prior results on the disciplinary effects of internal competition, and they constitute novel evidence that FinTech-engendered competition is a force upending the investment research industry.[6]

Our study also fits well in a broader literature on competition and product quality in information markets. Becker and Milbourn (2011), Doherty, Kartasheva, and Phillips (2012), and Xia (2014) examine the credit rating market—a highly regulated, non-competitive market where new entrants largely mirror the incumbents in their organization and practices. In contrast, we study a much less regulated and more competitive market, focusing on an entrant (Estimize) with a novel and distinct business model. Our work also supplements Gentzkow and Shapiro (2008) and Gentzkow, Glaeser, and Goldin (2006), who focus on the market for news. Our conclusion that technology-engendered competition to sell-side research suppliers reduces sell-side bias echoes Gentzkow et al.'s (2006) result that technology-engendered competition among newspapers in the 19th century reduces newspaper bias.

Our study has important policy implications. In the past two decades, regulators have addressed concerns about the adverse consequences of biased sell-side research (e.g., inefficient prices and wealth transfers from less sophisticated to more

[4] As a further test of the effort hypothesis, we consider four additional research attributes that should benefit from increased effort: informativeness, timeliness, forecast frequency, and conference call question length. We find that only timeliness improves.

[5] Hong and Kacperczyk (2010) find that individual analyst bias increases when the number of analysts covering a stock decreases due to broker mergers. Consistent with the notion that analysts compete at the industry level, Merkley et al. (2017) find that individual analyst bias increases when the number of analysts in the analyst industry declines, controlling for the number of analysts covering the stock.

[6] Another related stream of studies explores how time-varying demand and supply factors shape the nature of analyst research over time, resulting in greater emphasis on special services (Green, Jame, Markov, and Subasi 2014a), increased likelihood of issuing earnings forecasts for multiple firms on the same day (Drake, Joos, Pacelli, and Twedt 2020), and increased focus on industry-specific key performance indicators (Givoly, Li, Lourie, and Nekrasov 2019).

sophisticated investors) by comprehensively reforming sell-side analyst activities and communications with investment bankers and by requiring extensive conflict of interest disclosures. These regulations have reduced analyst bias, but at the cost of lower analyst coverage and lower research informativeness (Kadan et al. 2009). Our findings suggest that new forms of competition may be effective in reducing investor reliance on the sell-side and in constraining sell-side bias, without the unintended adverse consequences of traditional regulatory approaches.

II. BACKGROUND AND PREDICTIONS

FinTechs

Financial Technology (FinTech) firms combine "innovative business models and technology to enable, enhance, and disrupt financial services" (Ernst & Young [EY] 2019, 5). According to a recent survey, close to 90 percent of the executives in the financial services fear that their business is at risk to standalone FinTech companies (PricewaterhouseCoopers [PwC] 2017). Their fears seem justified, as another recent survey finds that one or more FinTech services (EY 2019) have been adopted by 64 percent of the consumers. Because FinTechs are a relatively recent phenomenon and data are scarce, the effects of FinTechs on the behavior of existing providers of financial services have remained largely unexplored.

In this study, we examine the effects of FinTechs on the quality of sell-side analyst research. We focus on sell-side analysts because of their crucial information intermediary role in capital markets and extant evidence that their research is tainted by biases, resulting in market mispricing (Dechow and Sloan 1997; Veenman and Verwijmeren 2018) and wealth transfers from less sophisticated to more sophisticated investors (Malmendier and Shanthikumar 2007; De Franco et al. 2007). In addition, rich observable data make measurement of research quality and tests of changes in quality possible. Our main tests focus on consensus earnings forecast bias and consensus earnings forecast accuracy. The consensus earnings forecast is a widely used measure of market expectations: bias (earnings minus the consensus) and accuracy (the absolute value of this difference) are its two most important attributes.[7]

The set of FinTechs that may affect the quality of sell-side analyst research is surprisingly large. Grennan and Michaely (2020) identify 290 FinTechs that compete directly or indirectly with sell-side analysts by conducting five non-mutually exclusive activities: (1) aggregation and synthesis of data from financial experts, (2) aggregation of financial news, (3) crowdsourcing investment research, (4) mining financial analysis and news for investment signals, and (5) ranking and evaluating investment research. They find that greater financial blogging is associated with greater price informativeness and that increased use of FinTech websites by users is associated with reduced informativeness of analyst research. We focus on Estimize, a notable provider of crowdsourced earnings forecasts because it poses a specific competitive threat to sell-side analysts, and because it has unique institutional features helpful in identifying the effect of FinTech competition on sell-side research (which we discuss in the next section).

We acknowledge that different types of FinTechs may enhance analyst research through different channels, and that one type of FinTech may compete against another type. For example, start-ups that rank analysts based on research quality (e.g., TipRank) do not directly compete with sell-side analysts, but may still discipline them by helping investors assess differences in analyst quality. On the other hand, research providers that aim to replace human intelligence with artificial intelligence may pose a competitive threat to both analysts and providers of crowdsourced research. Coleman et al.'s (2021) findings that recommendations produced with minimal involvement of humans are less biased and more profitable than analyst recommendations suggest that this threat is indeed looming.

Estimize

Estimize is an open platform that crowdsources earnings forecasts from a diverse set of contributors: buy-side and sell-side analysts, portfolio managers, retail investors, corporate finance professionals, industry experts, and students. As of December 2015, Estimize has attracted forecasts from over 15,000 contributors, covering more than 2,000 firms.[8] Estimize has received significant public acclaim and is frequently listed among the top FinTech companies.[9] Estimize forecasts are available on Bloomberg and several financial research platforms, regularly referenced in prominent financial media sources such as *Forbes, Barron's,* and the *Wall Street Journal,* and sold as a real-time feed to buy-side clients.

[7] See a recent review of the literature on analyst forecasts by Kothari, So, and Verdi (2016, Section Two, the first and second subsections) for a discussion of prior work on accuracy and bias, respectively.
[8] Estimize has experienced dramatic growth since the end of our sample period. As of December 2019, Estimize has over 90,000 unique contributors.
[9] See, for example: https://www.benzinga.com/news/15/04/5395774/the-2015-benzinga-fintech-award-winners

Estimize was founded by Leigh Drogen, a former hedge fund analyst, with the objective of "disrupting the whole sell-side analyst regime."[10] Drogen believed that crowdsourcing estimates would yield valuable information so long as the crowd comprises forecasters who have varied backgrounds and are free of sell-side analysts' conflicts of interests.[11] Jame et al. (2016) find evidence consistent with this reasoning: Estimize forecasts are significantly less biased than sell-side forecasts, more closely represent the market's earnings expectation, and are incrementally useful in forecasting earnings. In sum, Estimize is a competing provider of earnings forecasts whose arrival may encourage sell-side analysts to alter their forecasting behavior.

Estimize presents a unique opportunity to identify the FinTech effect from the effects of broad market and regulatory forces. Specifically, since Estimize primarily provides short-term earnings forecasts, sell-side forecasts with longer horizons and stock recommendations can be used in placebo tests to alleviate the concern that the arrival of Estimize coincides with a major market event. The FinTech hypothesis predicts improved quality only for short-term forecasts, whereas the alternative hypothesis predicts an across the board improvement in the quality of all sell-side research outputs.

Predictions and Mechanisms

Broadly, there are two mechanisms through which competition can increase consensus forecast quality. First, by exposing incumbents' inferior quality, new competition can trigger quality-increasing actions by the incumbents (Gentzkow and Shapiro 2008; Hong and Kacperczyk 2010; Xia 2014; Merkley et al. 2017). Second, the incumbent can use the new information produced by the entrant to improve the quality of their own forecasts. Next, we discuss how we expect these mechanisms to operate in our setting.

The Effect of Estimize on Consensus Bias

It is well-known that sell-side analysts bias their forecasts downward at the end of the quarter to help managers boost their stock price by reporting a positive earnings surprise, and that the market is unable to fully unravel this particular form of earnings management.[12] In choosing a level of bias, analysts trade off benefits in the form of greater investment banking revenues (Lin and McNichols 1998; Michaely and Womack 1999), access to management (Mayew 2008; Bradley, Jame, and Williams 2022), and likelihood of being hired by the firm in the future (Horton, Serafeim, and Wu 2017; Lourie 2019) with the costs of worsened reputations and long-term career prospects (Fang and Yasuda 2009; Altınkılıç, Balashov, and Hansen 2019).

Estimize has several properties that make it useful in exposing bias. In particular, Estimize forecasts are close to unbiased (Jame et al. 2016), accessible to investors at low cost, and collocated with sell-side forecasts on the Estimize site. This should make bias easier to unravel and, therefore, costlier by increasing expected reputational penalties and worsening career outcomes (Fang and Yasuda 2009) and even reducing the demand for analyst research. Serafeim, Horton, and Wu (2015, para. 4) observe that dissatisfaction with sell-side bias "explains why an increasing number of investors are conducting their own in-house analysis and rely more on the 'wisdom of the crowds' by using signals that are generated by web-based technologies that aggregate individual opinions and measure the sentiment of people towards a company." We suggest that the arrival of an unbiased competitor, Estimize, may have played a part in stoking up this dissatisfaction.

Furthermore, early evidence suggests that Estimize erodes the benefits of analyst bias. Schafhäutle and Veenman (2021) report that the market reaction to meeting or beating the sell-side consensus is lower when reported earnings fail to meet the Estimize consensus, consistent with Estimize raising investor awareness of analyst bias. Furthermore, unable to benefit from analysts' short-term pessimism, firms are likely to provide less information and investment banking business to biased analysts, the effect of which would be to make bias less beneficial to analysts.[13] In sum, Estimize encourages analysts to reduce strategic bias by making it more costly, as well as less beneficial. We refer to this mechanism as strategic bias mitigation.

The Effect of Estimize on Consensus Accuracy

Prior evidence suggests that accuracy is beneficial to analysts and brokerages. Accurate analysts are more likely to be voted as All-Stars (Stickel 1992) and promoted to higher-status brokerage firms or hedge funds (Hong and Kubik 2003; Cen,

[10] See: https://www.businessinsider.com/estimize-interview-leigh-drogan-2011-12

[11] In particular, Drogen highlights his dissatisfaction with the sell-side's "tendency to skew estimates in favor of higher earnings beat rates for the companies they cover"; see: https://www.estimize.com/beliefs

[12] Veenman and Verwijmeren (2018) show that greater predictable analyst pessimism is associated with greater earnings announcement returns. Relatedly, Ma and Markov (2017) show that the objective probability of meeting or beating the consensus (conditional on past information) differs from the subjective probability of meeting or beating the consensus (implied in earnings announcement returns).

[13] Concurrent studies use a similar diff-in-diff design to examine the effects of Estimize on firms. Sul (2020) finds that firms gaining Estimize coverage are more likely to engage in real earnings management and to provide more guidance, whereas Ott, Subramanyam, and Zhang (2020) find that these firms are more likely to engage in real earnings management and accruals earnings management.

Ornthanalai, and Schiller 2017), and less likely to lose their jobs (Mikhail, Walther, and Willis 1999); furthermore, more accurate research generates both greater price reactions (Park and Stice 2000; Chen, Francis, and Jiang 2005) and greater trading commissions (Jackson 2005). Increasing accuracy requires greater analyst effort and greater analyst resources in the form of support staff and databases. In equilibrium, the marginal benefit of further increasing accuracy is offset by the marginal cost of doing so.

It is unclear whether competition from Estimize would induce increased analyst effort and resources. Despite a sizeable timing advantage, Estimize forecasts are generally not more accurate than analyst forecasts, which weakens their ability to expose analysts as inaccurate and, as a consequence, spur accuracy-increasing actions. On the other hand, if the arrival of Estimize makes the long-term threat from FinTech more salient, then analysts may exert greater effort as a way to discourage further investments by Estimize and other FinTechs.

It is also possible that sell-side analysts improve their forecast accuracy by learning from Estimize forecasts. Jame et al. (2016) find that Estimize forecasts are incrementally useful in forecasting earnings, which would make them a valuable learning resource for the sell-side, but they also find that these forecasts are typically unavailable when sell-side forecasts are issued, which naturally diminishes their value as a learning resource.

Last, Estimize may improve forecast accuracy indirectly by discouraging strategic bias. This would not in itself guarantee an increase in accuracy. The reason is that the beneficial effect of a decline in bias may be offset by the adverse effect of losing access to management (Lim 2001), or it may be too small to reliably measure.[14]

In sum, there are three potential mechanisms through which Estimize may influence sell-side analyst accuracy: (1) greater effort,[15] (2) learning from Estimize forecasts, and (3) reducing strategic bias. To shed light on the second and third mechanisms, we exploit variation in incentives and opportunities to learn from Estimize forecasts at the individual analyst level. To shed light on the first mechanism, we consider additional research attributes likely to benefit from increased effort.

III. DATA AND DESCRIPTIVE STATISTICS

Sample Selection and Summary Statistics

In order to reliably measure the change in sell-side research quality around the introduction of Estimize in 2012, we focus on firms with continuous sell-side coverage from 2009 to 2015, which we define as availability of earnings forecasts in the I/B/E/S detail file. We require that these firms have a non-missing book value of equity and a stock price above $5 in the year prior to the arrival of Estimize. Our final sample includes 1,842 firms.

We obtain a dataset of Estimize forecasts of earnings that are announced in the period from January 2012 through December 2015. For each forecast, the dataset contains the forecasted earnings per share, the date of the forecast, the actual earnings per share, the date of the earnings announcement, the firm's stock ticker, and a unique ID for each contributor. Panel A of Table 1 provides summary statistics regarding the breadth and depth of Estimize coverage. Of the 1,842 firms in our sample, 1,391 have at least one Estimize forecast during the sample period. Collectively, there are 172,566 forecasts made by 11,167 unique contributors. The mean (median) Estimize firm is covered by 9.1 (4.0) different contributors during a quarter. Estimize's coverage and contributor base have significantly grown over time: from 2012 to 2015, the number of firm-quarters with forecasts increased from 1,694 to 5,011 and the number of contributors rose from 1,370 to 7,555.

Panel B of Table 1 examines the characteristics of firms added to Estimize at different times. All characteristics are measured in the post-Estimize period. We observe that firms added in 2012 are larger, have greater sell-side coverage, and are more growth-oriented (i.e., lower book-to-market ratios) than firms added in subsequent years. These firms also attract greater Estimize coverage: 11.4 contributors per quarter, compared to less than 2.5 contributors per quarter for later Estimize additions.

Panel C of Table 1 examines the breadth and depth of I/B/E/S coverage for the 1,391 firms covered by Estimize. In contrast with the rapid growth of Estimize contributors documented in Panel A, the total number of I/B/E/S analysts issuing forecasts for this sample declines from 2,930 in 2012 to 2,679 in 2015. The sample mean (median) number of I/B/E/S analysts per firm-quarter is 11.6 (10.0), whereas the mean (median) number of Estimize contributors is 9.1 (4.0), suggesting that Estimize coverage may be large enough to influence the properties of analyst coverage.

---

[14] There are two reasons why investors may care about analyst bias independent of its relation to accuracy. First, analyst bias may indicate a general tendency for taking actions harmful to clients; e.g., sharing research with select clients before release (Irvine, Lipson, and Puckett 2007), or biasing and delaying research to increase the profitability of analysts' own trades (Chan, Lin, Yu, and Zhao 2018). Second, investors who buy after long-term optimistic forecasts are issued and sell after this optimism turns into short-term pessimism, but before earnings are announced, are harmed by analyst bias even when managers reward biased analysts with information.

[15] The first mechanism encompasses costly actions by analysts (greater effort) or brokerage firms (greater analyst resources). For brevity, we refer to the first mechanism as "greater effort."

TABLE 1

Estimize Summary Statistics

Panel A: Breadth and Depth of Estimize Coverage

Year	Firms Covered	Firm-Quarters	Contributors	Forecasts	Contributors per Firm-Quarter Mean	Contributors per Firm-Quarter Median	Average Firms Followed
All (2012–2015)	1,391	15,120	11,167	172,566	9.05	4	8.06
2012	772	1,694	1,370	13,007	6.61	3	6.42
2013	1,271	3,781	1,612	24,750	5.88	3	9.67
2014	1,326	4,634	2,167	44,457	7.88	3	10.61
2015	1,362	5,011	7,555	90,352	13.82	6	7.05

Panel B: Characteristics of Firms Covered by Estimize

	Firms	Contributors per Firm-Quarter Average	Contributors per Firm-Quarter Median	Pct. Quarters with Coverage	Average Firm Characteristics I/B/E/S Coverage	Average Firm Characteristics Market Cap ($Bil)	Average Firm Characteristics Book-to-Market
2012 Additions	772	11.43	5.00	91.05%	14.37	18.59	0.39
2013 Additions	509	2.48	2.00	76.38%	8.72	3.72	0.52
2014 Additions	74	1.57	0.00	48.48%	6.09	2.02	0.38
2015 Additions	36	1.06	0.00	12.81%	5.67	1.14	0.44
Not on Estimize	451	0.00	0.00	0.00%	5.48	3.13	0.63

Panel C: Breadth and Depth of I/B/E/S Coverage

Year	Firms Covered	Firm-Quarters	Analysts	Forecasts	Analysts per Firm-Quarter Mean	Analysts per Firm-Quarter Median	Average Firms Followed
All (2012–2015)	1,391	21,990	4,044	370,183	11.64	10	6.68
2012	1,389	5,524	2,930	93,486	11.49	10	6.37
2013	1,390	5,514	2,827	92,487	11.78	10	6.72
2014	1,387	5,516	2,795	91,776	11.74	10	6.80
2015	1,391	5,436	2,679	92,434	11.56	10	6.82

Table 1 reports summary statistics for the set of forecasts contributed to Estimize from January 2012 to December 2015 and the set of firms added to Estimize over the same period. Panel A reports the breadth (Firms Covered) and depth of Estimize coverage (Contributors per Firm-Quarter) over time. Panel B partitions Estimize firms based on when they are added to Estimize and reports summary statistics for each group. Panel C examines the breadth and depth of I/B/E/S coverage of the firms covered by Estimize. The sample includes 1,842 firms with (1) continuous sell-side coverage from 2009 to 2015, (2) a stock price of at least $5 at the end of 2011, and (3) non-missing book value of equity at the end of 2011.

The Properties of Estimize and I/B/E/S Quarterly Forecasts

We construct two measures of consensus forecast quality: forecast bias, *Bias,* and forecast accuracy, *AbsFE*. To construct the sell-side, or the Estimize, consensus, we include the most recent forecast issued by an analyst, or a contributor, within 120 days of the earnings announcement date (i.e., the one-quarter-ahead forecast). These forecasts account for approximately 22 percent of all sell-side forecasts and 93 percent of all Estimize forecasts (untabulated for brevity), underscoring the difference in forecast horizons between the two service providers. We exclude forecasts flagged by Estimize as unreliable (roughly 1 percent of the sample).

We compute *Bias* as the consensus forecast error, scaled by price:

$$Bias_{jt} = \frac{Actual_{jt} - ConsF_{jt}}{Price_{jt-1}} * 100 \tag{1}$$

TABLE 2

Characteristics of Estimize and I/B/E/S Quarterly Forecasts

Panel A: Estimize Forecasts

	N	Mean	Median	Std. Dev.	25th	75th
Coverage	8,268	12.55	6.00	26.02	3.00	13.00
Forecast Age	8,268	9.74	6.33	11.48	2.00	13.64
Bias	8,268	0.01	0.01	0.28	−0.05	0.10
AbsFE	8,268	0.17	0.08	0.23	0.03	0.19

Panel B: I/B/E/S Forecasts

	N	Mean	Median	Std. Dev.	25th	75th
Coverage	9,082	14.37	13.00	8.19	8.00	19.00
Forecast Age	9,082	63.79	66.79	21.58	48.83	80.00
Bias	9,082	0.05	0.04	0.39	−0.02	0.14
AbsFE	9,082	0.20	0.08	0.34	0.03	0.21

Table 2 examines key attributes of Estimize and I/B/E/S one-quarter-ahead forecasts. For each attribute, we compute the mean of the measure across all analysts covering the firm in a quarter. We use the most recent forecast by a contributor or an analyst. We exclude forecasts flagged as unreliable by Estimize, and we limit the sample to forecasts issued with 120 days of the earnings announcement. We report the mean, median, standard deviation, and 25th and 75th percentiles for each attribute across all firm-quarters in the sample. The sample includes 772 firms added to Estimize in 2012 and covered by I/B/E/S analysts and Estimize contributors from 2013 to 2015.
Detailed definitions for each attribute are provided in Appendix A.

where $Actual_{jt}$ is reported earnings for firm j in quarter t; $ConsF_{jt}$ is the corresponding consensus forecast (calculated by averaging individual analyst forecasts, F_{ijt}, where i is an analyst index); and $Price_{jt-1}$ is the closing stock price at the end of the prior year. Positive (negative) bias corresponds to analyst pessimism (optimism). We winsorize *Bias* at the 2.5th and the 97.5th percentiles to reduce the effect of extreme observations.[16] We calculate *AbsFE* as the absolute value of *Bias*.

Panels A and B of Table 2 compare Estimize and I/B/E/S forecasts issued from 2013 to 2015 on four dimensions: *Bias* and *AbsFE,* as defined above, and *Coverage* and *Forecast Age,* defined as the number of contributors (analysts) providing forecasts in a firm-quarter and the average age of the forecasts that make up the Estimize (I/B/E/S) consensus, respectively. The sample firms are those added to Estimize in 2012 (see Panel B of Table 1). This sample choice foreshadows the *First-Year Treatment* sample used in much of the subsequent analyses, where we define firms added to Estimize in 2012 as "treated firms" and the 2013–2015 period as the "post-event window."

The mean number of Estimize contributors is comparable to the mean number of sell-side analysts (12.55 versus 14.37) in Table 2, but the median number of Estimize contributors is significantly lower (6 versus 13), consistent with large skewness in the distribution of Estimize coverage. The difference in forecast age between Estimize forecasts and sell-side forecasts is striking. For example, the median Estimize forecast is issued less than a week prior to the earnings announcement (6.33 days), while the median sell-side forecast is issued more than two months prior (66.79 days).

The location of the distribution of Estimize *Bias* is essentially 0 (mean and median of 0.01) in Table 2, whereas the location of the distribution of I/B/E/S *Bias* is above 0 (mean of 0.05 and median of 0.04), consistent with Estimize forecasts being largely unbiased and I/B/E/S forecasts being pessimistic. Despite the fact that Estimize forecasts are issued weeks after sell-side forecasts are issued, Estimize forecasts are similarly accurate: The distributions of *AbsFE* in the Estimize and sell-side samples exhibit similar means of 0.17 and 0.20, respectively, and identical medians of 0.08. The fact that Estimize forecasts are less biased than sell-side forecasts, but exhibit similar accuracy despite their substantial timing advantage, suggests that sell-side analysts face more pressure to reduce short-term bias than to increase accuracy. On the other hand, we acknowledge that if reducing bias is highly detrimental to analyst relationships with management, then it is possible that some analysts may choose to increase accuracy, rather than decrease bias, in response to the arrival of Estimize.

[16] Winsorizing *Bias* at the 1st and the 99th percentiles results in significant sample kurtosis (10). As a result, our difference-in-differences estimates have similar magnitudes, but slightly larger standard errors.

IV. EMPIRICAL ANALYSES

Research Design

Our central prediction is that Estimize forecasts, being easily accessible, reasonably accurate, and substantially less biased, place pressure on sell-side analysts to improve the quality of their research. To test this prediction, we conduct a difference-in-differences analysis, comparing changes in the quality of sell-side coverage of treated and control firms around a treatment date.

We implement the diff-in-diff analysis in two ways. Our first approach defines treated firms as those added to Estimize in 2012 (*First-Year Treatment*) and the pre- and post-event windows from 2009 to 2011 and 2013 to 2015, respectively. In our second approach, treated firms are those added to Estimize in 2012, 2013, or 2014, and covered in at least 25 percent of the post-event quarters (*Staggered Treatment*). The reason for this coverage requirement is to exclude firms where sell-side analysts face no or weak competitive pressure due to sparse Estimize coverage. We also reduce the pre- and post-event windows to two years (eight quarters) around the event. We note that for firms added in 2014, the post-event window ends in the fourth quarter of 2015 because this is the last quarter that we can determine whether a firm is covered by Estimize—a prerequisite for separating treated firms and control firms and conducting the diff-in-diff analysis.

An advantage of the second approach is that it exploits the staggered initiation of coverage by Estimize to alleviate the concern that a single shock in 2012 drives our findings. Its disadvantage is that it includes many firms with relatively limited Estimize coverage. In particular, the average number of contributors in a firm-quarter for firms added in 2013 and 2014 is less than 2.50, roughly 20 percent of the mean Estimize coverage for firms in the *First-Year Treatment* sample (11.43). This suggests that, on average, analysts covering firms in the *Staggered Treatment* sample face weaker competitive pressure than analysts covering firms in the *First-Year Treatment* sample.[17]

Candidate control firms include those not covered by Estimize at any point during the sample period. A natural concern is that systematic differences in covariates between treated and non-treated firms create differences in how forecast quality changes in the two groups of firms, biasing our difference-in-differences estimates. With the functional relation between covariates and forecast quality generally unknown, we control for confounding covariates by implementing the propensity score matching (PSM) method.

The basic idea behind PSM is to estimate and equate the probabilities of receiving the treatment as a function of confounding covariates. Based on the evidence from Jame et al. (2016, Table 2), we expect that Estimize coverage will be increasing in *Log* (*Size*), *Log* (*I/B/E/S Coverage*), and *Log* (*Turnover*) and decreasing in *Book-to-Market*. We also conjecture that Estimize contributors will shy away from covering firms whose earnings are difficult to forecast, as proxied by the *AbsFE* of the sell-side consensus. Finally, we explore whether Estimize coverage is related to past sell-side bias (*Bias*). We measure firm characteristics in the year prior to the treatment, and forecast characteristics as quarterly averages over the three years prior to treatment. We standardize all independent variables to have mean 0 and unit variance.

Specification 1 of Table 3 reports the odds ratios of the logistic regression for the *First-Year Treatment* sample. We find that the likelihood that a firm is added to Estimize in 2012 increases with *Size, I/B/E/S Coverage,* and *Turnover,* and decreases with *Book-to-Market*.[18] For example, Specification 1 shows that a one-standard-deviation increase in *Log* (*Size*) is associated with a 123 percent increase in the likelihood of being added to Estimize, while a one-standard-deviation increase in *Book-to-Market* is associated with a 56 percent decline in the likelihood of being added to Estimize. We also find that firms added to Estimize in 2012 have smaller *AbsFE,* consistent with Estimize contributors preferring stocks whose earnings are easier to forecast. Finally, we document a positive relation between 2012 additions and past sell-side bias, consistent with some contributors gravitating toward firms where sell-side analysts tend to be more pessimistic.

We next match each treated firm to one control firm with replacement using nearest neighbor matching. Specification 2 of Table 3 repeats the logistic regression after limiting the sample to the treated firms and propensity score matched control firms. Matching substantially improves covariate balance. The largest t-statistic in Specification 2 (in absolute value) is 1.82, compared to 8.38 prior to matching.

Specifications 3 and 4 of Table 3 present analogous results for the *Staggered Treatment* sample. Using this expanded sample yields qualitatively similar conclusions. In particular, the *Staggered Treatment* sample is positively correlated with *Size, I/B/E/S Coverage, Turnover,* and *Bias* and negatively correlated with *Book-to-Market* and *AbsFE,* and the matched sample results in substantial improvements in covariate balance.

---

[17] Furthermore, in untabulated analysis, we find that the quality of Estimize coverage received by firms in the *Staggered Treatment* sample is generally lower than that of firms in the *First-Year Treatment* sample. For example, Estimize coverage of firms in the *Staggered Treatment* sample is less biased than sell-side coverage of these firms in 62 percent of all firm-quarters; the corresponding figure for the smaller *First-Year Treatment* sample is 70 percent.

[18] Our findings are consistent with the results tabulated in Jame et al. (2016, Internet Appendix).

TABLE 3

Propensity Score Matching

	First-Year Treatment		*Staggered Treatment*	
	Unmatched [1]	**Matched [2]**	**Unmatched [3]**	**Matched [4]**
Log (Size)	2.23***	0.75	1.81***	0.91
	(6.07)	(−1.32)	(5.42)	(−0.53)
Book-to-Market	0.44***	0.84	0.53***	0.84
	(−8.38)	(−1.00)	(−8.48)	(−1.19)
Log (I/B/E/S Coverage)	1.74***	1.46*	1.58***	1.20
	(4.20)	(1.82)	(4.43)	(1.10)
Log (Turnover)	1.84***	0.83	1.81***	0.85
	(5.34)	(−1.02)	(5.67)	(−1.08)
AbsFE	0.50***	0.93	0.53***	0.96
	(−6.72)	(−0.36)	(−7.59)	(−0.25)
Bias	1.45***	1.27*	1.31***	1.19
	(4.53)	(1.73)	(4.06)	(1.49)
Percent Concordant	90.2%	62.0%	87.2%	60%
Obs. (Firms)	1,252	1,544	1,624	2,288
Treated Firms	772	772	1,144	1,144
Propensity Score Treated	80.49%	80.49%	82.11%	82.11%
Propensity Score Control	31.36%	80.33%	42.64%	82.02%

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 3 reports odds ratios from a logistic regression where the dependent variable is an indicator variable equal to 1 if the firm is a treated firm, and 0 otherwise. In Specifications 1 and 2, we define a firm as treated if it is added to Estimize in 2012 (*First-Year Treatment*). In Specifications 3 and 4, we define a firm as treated if it is added to Estimize at any point prior to 2015, and it has a consensus forecast available on Estimize in at least 25 percent of the quarters after being added to Estimize (*Staggered Treatment*). In Specifications 1 and 3, the sample includes treated firms and all possible control firms, defined as any firm not added to Estimize as of 2015 (*Unmatched Sample*). In Specifications 2 and 4, the sample is limited to treated firms and matched control firms. We match each treated firm to one control firm with the most similar probability of being treated. We estimate the probability of being treated as a function of *Log* (*Size*), *Book-to-Market, Log* (*I/B/E/S Coverage*), *Log* (*Turnover*), *AbsFE,* and *Bias*. We measure firm characteristics in the year prior to treatment, and forecast characteristics as quarterly averages over the three years prior to treatment. We standardize all independent variables to have mean 0 and unit variance, and we report z-statistics based on heteroscedasticity-robust standard errors in parentheses.
We include detailed definitions in Appendix A.

Changes in Consensus Forecast Quality

First-Year Treatment Sample

We examine the effect of Estimize competition on the quality of the sell-side consensus using difference-in-differences regressions. We begin by estimating the following model for the *First-Year Treatment* sample:

$$ConsFQ_{jt} = \beta_1 Post \times Treated_{jt} + Controls_{jt} + \psi_j + \delta_t + \varepsilon_{jt}, \quad (2)$$

where *ConsFQ* is sell-side consensus forecast quality, defined as either *Bias* or *AbsFE*. *Treated* is equal to 1 (0) for treated (matched control) firms. *Post* is equal to 1 (0) for all quarters from 2013 to 2015 (2009 to 2011). Thus, β_1 captures the change in forecast quality for treated firms relative to control firms after the introduction of Estimize. Control variables include *Log* (*Size*), *Book-to-Market, Guidance, Log* (*I/B/E/S Coverage*), *Log* (*Turnover*), *Log* (*Volatility*), *Return,* and *Log* (*Forecast Age*) (see Appendix A for detailed definitions). ψ_j and δ_t denote firm and calendar quarter fixed effects. We do not include *Post* and *Treated* as separate regressors because they are subsumed by calendar quarter and firm fixed effects, respectively. We standardize all continuous variables, including the dependent variables, to have mean 0 and standard deviation of 1 for each firm. We cluster standard errors by firm and by calendar quarter. The sample includes 36,511 firm-quarters from 2009 through 2015, excluding 2012 (the event year).

Specifications 1 and 2 of Table 4 report the results for *Bias* and *AbsFE,* respectively. As expected, we find a decline in *Bias* for treated firms after the introduction of Estimize relative to matched control firms. The diff-in-diff estimate in Specification 1

TABLE 4

Changes in Consensus Forecast Quality
First Year Treatment

	Bias [1]	***AbsFE*** [2]	***Bias*** [3]	***AbsFE*** [4]
Post × Treated	−24.57%***	−5.70%		
	(−3.52)	(−0.88)		
Post_2013 × Treated			−16.71%**	−11.12%
			(−2.04)	(−1.49)
Post_2014 × Treated			−31.21%***	−10.74%
			(−4.62)	(−1.16)
Post_2015 × Treated			−26.85%***	4.70%
			(−2.97)	(0.53)
Log (Size)	−12.15%***	−25.80%***	−11.96%***	−26.00%***
	(−3.76)	(−6.02)	(−3.66)	(−8.78)
Book-to-Market	3.90%**	6.90%***	3.99%**	7.09%***
	(2.28)	(2.75)	(2.31)	(3.75)
Guidance	−1.04%	−20.50%***	−1.10%	−20.53%***
	(−0.23)	(−5.44)	(−0.21)	(−5.46)
Log (I/B/E/S Coverage)	−1.15%	0.20%	−0.87%	0.03%
	(−0.46)	(0.09)	(−0.49)	(0.02)
Log (Turnover)	−5.58%***	−4.10%**	−5.65%***	−3.79%*
	(−3.84)	(−1.97)	(−3.61)	(−1.84)
Log (Volatility)	6.99%	9.10%**	6.60%**	9.02%***
	(1.23)	(2.55)	(2.19)	(2.60)
Return	−1.43%	−7.00%***	−1.10%	−6.77%***
	(−0.61)	(−3.74)	(−0.59)	(−3.95)
Log (Forecast Age)	3.96%*	−2.40%*	3.99%***	−2.45%*
	(1.89)	(−1.83)	(4.19)	(−1.81)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Calendar Quarter FE	Yes	Yes	Yes	Yes
R^2	5.83%	17.41%	5.88%	17.48%
Obs. (Firm-Quarters)	36,511	36,511	36,511	36,511

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 4 reports results from the following ordinary least squares (OLS) regression:

$$ConsFQ_{jt} = \beta_1 Post \times Treated_{jt} + Controls_{jt} + \psi_j + \delta_t + \varepsilon_{jt}.$$

The dependent variable, $ConsFQ_{jt}$, is either $Bias_{jt}$ or $AbsFE_{jt}$. *Treated* is equal to 1 for firms added to Estimize in 2012, and 0 for matched control firms (as defined in Table 3). In Specifications 1 and 2, *Post* is equal to 1 in the post-period (2013 to 2015), and 0 during the pre-period (2009 to 2011). In Specifications 3 and 4, we replace *Post* with separate indicator variables for each post-treatment year: *Post_2013, Post_2014,* and *Post_2015*. *Post_2013* is equal to 1 for forecasts issued in 2013, and 0 otherwise. *Post_2014* and *Post_2015* are defined analogously. *Controls* includes *Size, Book-to-Market, Guidance, I/B/E/S Coverage, Turnover, Volatility, Return,* and *Forecast Age*. We standardize all continuous variables, including the dependent variables, to have mean 0 and standard deviation of 1 for each firm. ψ_j and δ_t denote firm and calendar quarter fixed effects, respectively. Reported t-statistics are based on standard errors double-clustered by firm and calendar quarter.
Detailed variable definitions are provided in Appendix A.

is highly statistically significant (t = −3.52) and economically large.[19] Multiplying this estimate (−24.57 percent) by the pre-event standard deviation of *Bias* (0.48) yields an average decline of −0.118, which is 84 percent of the mean pre-event *Bias* (0.141). The diff-in-diff estimate in Specification 2 is statistically insignificant, suggesting that Estimize competition does not lead to widespread improvements in sell-side consensus accuracy.[20]

[19] The statistical significance is robust to alternative clustering choices, such as clustering by firm (t = −3.86) or by calendar quarter (t = −3.32).
[20] Theoretically, bias and accuracy are inversely related, meaning that bias reduction implies accuracy improvement. Empirically, the relation between bias and accuracy is relatively weak, which means that accuracy improvement may be too small to accurately measure. For example, the average absolute forecast error for treated firms in the pre-period marginally decreases from 0.326 to 0.315 after we de-bias these forecasts by subtracting the mean forecast error. In subsequent tests, we find significant improvements in accuracy when we focus on analysts who face stronger competitive pressure (Table 8).

In Specifications 3 and 4 of Table 4, we estimate the effect of Estimize on bias and accuracy separately for each of the three post-treatment years (2013, 2014, and 2015) after replacing *Post* with three indicator variables: *Post_2013, Post_2014,* and *Post_2015*. *Post_2013* is set equal to 1 for forecasts issued in 2013, and 0 otherwise. *Post_2014* and *Post_2015* are defined analogously. We find a statistically significant decline in bias of −16.7 percent, −31.2 percent, and −26.9 percent in 2013, 2014, and 2015, respectively. These diff-in-diff estimates have similar magnitudes, suggesting that the effect of Estimize on bias is fairly quick and long-lasting. We do not find that accuracy changes in any of the post-treatment years.

Staggered Treatment Sample

We next report difference-in-differences estimates for the *Staggered Treatment* sample. We modify Equation (2) as follows: We shorten the post- and pre-event periods by defining *Post* as 1 (0) in the eight-quarter period after (before) a firm is added to Estimize, and we drop the quarter in which a firm is added to the Estimize sample. We add *Post* as a regressor because the sample includes firms treated at different times, and the calendar quarter fixed effects no longer subsume the coefficient on *Post*. We report our findings in Table 5.

We continue to find significant improvements in consensus bias, but not in consensus accuracy. Our diff-in-diff estimate of the effect on consensus bias in Specification 1 of Table 5 is −20.55 percent, somewhat lower than the corresponding effect of −24.57 percent reported for the *First-Year Treatment* sample estimate in Specification 1 of Table 4. We suggest that the slightly weaker result in the *Staggered Treatment* sample is due to the fact that firms added after 2012 are covered by fewer Estimize contributors than firms added in 2012 (Panel B of Table 1). Multiplying the estimate of −20.55 percent by the pre-event standard deviation of *Bias* for the staggered treatment sample yields a decline equivalent to 82 percent of the mean pre-event *Bias,* which we still view as economically large. In Specifications 3 and 4, we replace the *Treated* indicator variable with a series of variables indicating the year in which a firm is added to Estimize: *Treated_2012, Treated_2013,* and *Treated_2014*. We find a greater decline in bias for firms treated in 2012 than in 2014 (−31.3 percent versus −12.7 percent), consistent with treatment intensity in 2012 being higher than treatment intensity in 2014 (Panel B of Table 1), although the difference is not statistically significant.

Next, we investigate how consensus forecast quality varies in the quarters that immediately precede or follow the event. We replace the indicator *Post* × *Treated* with 15 indicators, each identifying an event quarter: from Q_{t-8} to Q_{t-2} (pre-Estimize) and from Q_t to Q_{t+8} (post-Estimize). The coefficients on the included indicators measure changes in forecast quality relative to forecast quality for the excluded quarter (t−1).[21] We plot parameter estimates and 95 percent confidence intervals from our analyses of *Bias* and *AbsFE* in Figure 1, Panels A and B.

In Figure 1, Panel A, we observe that all nine post-Estimize quarter coefficients are negative, with six statistically significant at the 5 percent level: Q_{t+2} *to* Q_{t+7}. The coefficient on Q_t is the smallest, which is unsurprising: Most Estimize contributors issue forecasts toward the end of the quarter after sell-side analysts issue theirs, leaving sell side analysts little time to change their behavior. The decline in bias in quarter Q_{t+1} is comparable to the decline in bias in later quarters, but statistically significant at a lower significance level of 10 percent, which is again consistent with Estimize having a fairly quick and long-lasting effect on bias. In contrast, the coefficients on the pre-Estimize indicators are not statistically different from 0 or from each other, which suggests that our findings are unlikely to be attributable to pre-trends. The signs of the post-Estimize coefficients in Figure 1, Panel B do not exhibit any systematic pattern, thus confirming our earlier conclusion of no widespread improvements in sell-side accuracy.

Overall, our analyses of the *First-Year Treatment* (Table 4) and *Staggered Treatment* (Table 5) yield similar results. The arrival of Estimize leads to lower consensus forecast bias, but not to higher consensus forecast accuracy. In the interest of brevity, our remaining tests focus on the *First-Year Treatment* sample, where the treatment is, on average, stronger and, as a consequence, statistical power is higher.

Robustness Tests

In Table 6, we examine whether our Table 4 results are robust to key research design choices. For reference, we tabulate our baseline results from Table 4 in the first row of Table 6.

In Rows 2 and 3 of Table 6, we shrink the event window from 12 pre- and post-event quarters [−12,12] to either eight quarters [−8,8] or four quarters [−4,4]. The point estimate for *Bias* is similar over the [−8,8] window, but smaller and not reliably different from 0 over the [−4,4] window, where statistical power is the lowest.

Rows 4 through 7 of Table 6 consider alternative matching techniques. In Row 4, we confirm that the results are qualitatively similar if we estimate Equation (2) prior to conducting propensity score matching. In Row 5, we use propensity

[21] We define Q_t, the quarter in which a firm is added to Estimize, as a post-event quarter, although Q_t is technically the quarter of the event.

TABLE 5

Changes in Consensus Forecast Quality
Staggered Treatment

	Bias [1]	*AbsFE* [2]	*Bias* [3]	*AbsFE* [4]
Post × *Treated*	−20.55%***	−4.15%		
	(−2.71)	(−0.51)		
Post × *Treated_2012*			−31.25%*	−13.56%
			(−1.71)	(−0.68)
Post × *Treated_2013*			−25.27%***	−9.55%
			(−3.20)	(−1.00)
Post × *Treated_2014*			−12.68%**	4.09%
			(−2.16)	(0.63)
Log (*Size*)	−9.28%***	−19.52%***	24.78%**	12.90%
	(−3.75)	(−7.35)	(2.00)	(1.02)
Book-to-Market	3.00%	4.78%**	−9.30%***	−19.55%***
	(1.27)	(2.41)	(−3.52)	(−7.33)
Guidance	−9.43%**	−17.77%***	2.90%	4.68%**
	(−2.49)	(−3.91)	(1.59)	(2.54)
Log (*I/B/E/S Coverage*)	−8.06%***	−5.73%***	−9.31%**	−17.65%***
	(−3.11)	(−2.68)	(−2.30)	(−4.65)
Log (*Turnover*)	−5.97%***	−4.13%***	−8.23%***	−5.93%***
	(−3.81)	(−3.34)	(−3.52)	(−2.84)
Log (*Volatility*)	2.58%	6.56%***	−5.98%***	−4.12%***
	(0.78)	(2.73)	(−3.41)	(−2.73)
Return	−1.26%	−4.80%**	2.98%	6.98%***
	(−0.60)	(−2.13)	(0.98)	(2.58)
Log (*Forecast Age*)	2.36%	−1.89%	−1.45%	−5.01%***
	(1.31)	(−0.88)	(−0.79)	(−2.77)
Post	21.43%***	9.96%	2.39%	−1.86%
	(2.82)	(1.38)	(1.36)	(−1.06)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Calendar Quarter FE	Yes	Yes	Yes	Yes
R^2	4.15%	8.07%	4.21%	8.14%
Obs. (Firm-Quarters)	34,252	34,252	34,252	34,252

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 5 reports results from the following OLS regression:

$$ConsFQ_{jt} = \beta_1 Post \times Treated_{jt} + Controls_{jt} + \psi_j + \delta_t + \varepsilon_{jt}.$$

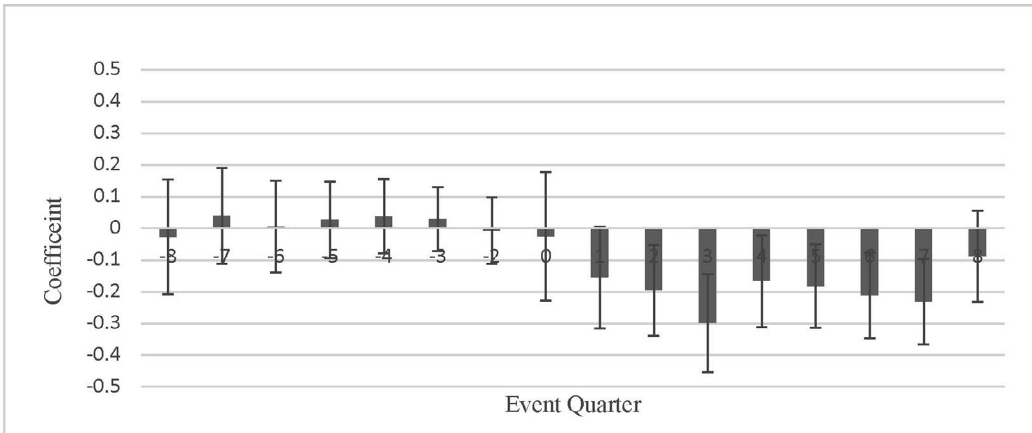
The dependent variable, $ConsFQ_{jt}$, is either $Bias_{jt}$ or $AbsFE_{jt}$. *Treated* equals 1 for firms that are added to Estimize at any point prior to 2015 and have Estimize consensus forecasts in at least 25 percent of the quarters after being added to Estimize, and 0 for matched control firms (as defined in Table 3). In Specifications 1 and 2, *Post* is equal to 1 (0) in the eight-quarter period after (before) Estimize initiates coverage. In Specifications 3 and 4, we replace *Treated* with three dummy variables indicating when the firm is first added to Estimize: *Treated_2012, Treated_2013,* and *Treated_2014*. *Treated_2012* equals 1 for firms added to Estimize in 2012. *Treated_2013* and *Treated_2014* are defined analogously. *Controls* includes *Size, Book-to-Market, Guidance, I/B/E/S Coverage, Turnover, Volatility, Return,* and *Forecast Age*. We standardize all continuous variables, including the dependent variables, to have mean 0 and standard deviation of 1 for each firm. ψ_j and δ_t denote firm and calendar quarter fixed effects, respectively. Reported t-statistics are based on standard errors double-clustered by firm and calendar quarter.
Detailed variable definitions are provided in Appendix A.

score matching, but require that the absolute value of the difference in propensity scores be less than 0.50 percent, eliminating 156 firms in the *First-Year Treatment* sample. Our results hold.

While the propensity score model matches on the probability of being treated, treated firms may still differ significantly with respect to an individual variable (e.g., a large-value firm could be matched with a small-growth firm). To address this concern, we identify matched control firms using coarsened exact matching (see, e.g., Iacus, King, and Porro 2012). This method matches on a coarsened range (or strata) of covariates rather than covariates' exact values to prevent sample attrition. In Row 6 of Table 6, we match on all six covariates after coarsening each into two strata using median breakpoints, and discard 80

FIGURE 1
Changes in Consensus Forecast Quality in Event Time

Panel A: *Bias*



Panel B: *AbsFE*

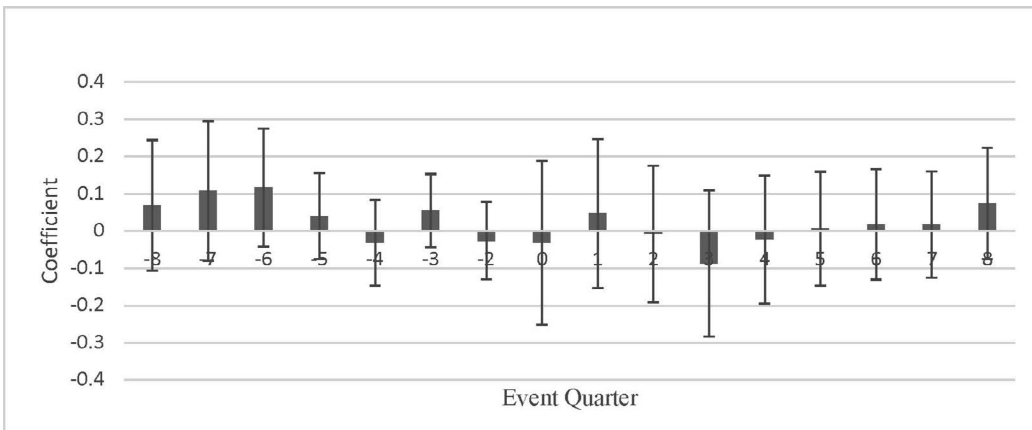


Figure 1 examines differences in forecast quality for treated and matched control firms after the arrival of Estimize in event time. Panels A and B repeat Specifications 3 and 4 of Table 4 after partitioning $Post \times Treated$ into its quarterly components: $Treated \times t{-}8$ through $Treated \times t{+}8$. The omitted quarter is $Treated \times t{-}1$. The error bars report the 95 percent confidence intervals based on standard errors clustered by firm.

firms that lack common support. We find slightly weaker, but still significant results. In Row 7, we use entropy balancing. By applying continuous weights to candidate control firms to equate the moments of the covariate distributions, entropy balancing retains all treated firms (Hainmueller 2012). We match on the first moment of the covariates and find similar results.

Rows 8 through 11 of Table 6 consider alternative measures of forecast bias (and, when appropriate, absolute forecast error). We find similar results when we use the median forecast rather than the mean (Row 8), weaker, but still significant, results when bias is defined as a meet-or-beat (*MBE*) indicator variable (Row 9), and slightly stronger results when using all available quarterly forecasts rather than relying on each analyst's last forecast of the quarter (Row 10). In Row 11, we conduct a placebo test to preclude the alternative explanation that Estimize firms experience a positive performance shock. Unlike our hypothesis, which predicts that only analyst forecast quality will be impacted, this alternative explanation also predicts a change in statistical forecast quality. We construct a statistical forecast of firm *j*'s quarter *t* earnings as:

$$Stat\_Fcst = earn_{jt-4} + \theta_{j0} + \theta_{jt}\left(earn_{jt-1} - earn_{jt-5}\right), \tag{3}$$

where $earn_{jt}$ is firm *j*'s quarter *t* earnings, and θ_{j0} and θ_{j1} are parameters of an autoregressive model in fourth difference

TABLE 6

Changes in Consensus Forecast Quality
Robustness

	Bias **[1]**	***AbsFE*** **[2]**
1. *Baseline*	−24.57%***	−5.70%
	(−3.52)	(−0.88)
Alternative Event Windows		
2. [−4,4]	−14.52%	−13.15%
	(−0.77)	(−1.29)
3. [−8,8]	−24.28%***	−10.91%**
	(−2.67)	(−2.32)
Alternative Matching		
4. *No Matching*	−25.34%***	3.09%
	(−4.07)	(0.95)
5. *PSM Matching—Common Support*	−23.18%***	−3.52%
	(−3.64)	(−0.64)
6. *Coarsened Exact Matching*	−18.23%***	−4.68%
	(−3.01)	(−0.71)
7. *Entropy Balancing*	−17.46%***	−4.21%
	(−2.77)	(−0.76)
Alternative Measures of Bias/Accuracy		
8. *Median Bias*	−21.17%***	−7.45%
	(−2.80)	(−1.17)
9. *MBE*	−7.78%**	NA
	(−2.06)	
10. *Replace last forecast with all forecasts*	−26.60%***	−6.63%
	(−4.39)	(−0.99)
11. *Statistical Measure—Placebo*	5.40%	−1.16%
	(0.82)	(−0.19)
Alternative Subsamples		
12. *Exclude Firms with Guidance*	−23.47%***	−5.40%
	(−3.21)	(−0.85)

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 6 examines the sensitivity of the difference-in-differences estimates in Table 4 (tabulated for convenience in Row 1) to alternative research design choices. In Rows 2 and 3, we report results for different symmetric event windows around the event quarter. In Row 4, we report results without matching. In Row 5, we use propensity score matching, but exclude 156 treated firms due to lack of common support, which we define as the absolute difference in propensity scores between the treated firm and matched control firm being greater than 0.50 percent. In Row 6, we identify control firms using coarsened exact matching. We match on all six covariates included in Table 3 after coarsening each into two strata based on median breakpoints. In Row 7, we implement the entropy balancing approach by matching on the first moment of all six covariates. In Row 8, we replace average *Bias* and *AbsFE* with *Median Bias* and *Median AbsFE*. In Row 9, we replace *Bias* with *MBE* (a meet-or-beat indicator variable). In Row 10, we compute the average *Bias* or *AbsFE* across all forecasts issued in the quarter rather than the last forecast issued by each analyst. In Row 11, we conduct a placebo test using a statistical forecast obtained from a first-order autoregressive process in fourth difference with a drift. In Row 12, we conduct the baseline analysis on a sample of firm-quarters without management guidance. The t-statistics (in parentheses) are based on standard errors double-clustered by firm and calendar quarter. Detailed variable definitions are available in Appendix A.

estimated on the past 30 quarters of data. We define *Statistical Bias* as actual earnings minus the statistical forecast, scaled by the lagged stock price, and we define *Statistical AbsFE* as the absolute value of *Statistical Bias*. We find no evidence that treated firms experience a decline in *Statistical Bias* (or *Statistical AbsFE*) relative to control firms.[22]

Finally, in Row 12 of Table 6, we confirm that our results hold in a subsample of firm-quarters in which management does not issue any earnings guidance. This alleviates the concern that, for unrelated reasons, managers guide down analysts less in the post-Estimize period, resulting in less pessimistic analyst forecasts.

[22] We find similar results when we compute expected earnings using a seasonal random walk with drift or a seasonal random walk without drift.

Placebo Tests: Changes in Bias for Longer-Horizon Forecasts and Recommendations

An alternative hypothesis is that reputational concerns or other broad forces shaping analyst incentives affect analysts covering treated firms more strongly than those covering matched control firms. This hypothesis predicts an improvement in quality not only for short-term earnings forecasts, but for all other research outputs. In contrast, our Estimize competition hypothesis predicts an improvement in quality only for short-term forecasts, since Estimize provides few longer-term forecasts and no stock recommendations.[23]

To rule out the alternative hypothesis, we examine the effects of Estimize on the bias of longer-term earnings forecasts and investment recommendations.[24] We focus on *Bias* of *t*-quarters-ahead earnings forecasts, $Bias_t$, where *t* ranges from five to eight quarters.[25] We also examine recommendation bias, measured as the average recommendation level at the end of each quarter (*Rec Level*). In computing *Rec Level,* we convert recommendations to a numeric value using the following five rankings: 1 for Strong Buy, 2 for Buy, 3 for Hold, 4 for Sell/Underperform, and 5 for Strong Sell.

Specifications 1 through 5 of Table 7 report the results for $Bias_5$, $Bias_6$, $Bias_7$, $Bias_8$, and *Rec Level,* respectively. In all five cases, the difference-in-differences estimate is economically small and statistically insignificant. The fact that bias declines for research outputs where analysts compete with Estimize, but not for outputs where they do not compete with Estimize, alleviates the concern that the arrival of Estimize coincides with unobserved market forces, which we expect to effect changes in all research outputs.

Intensity of Estimize Competition and Changes in Consensus Forecast Quality

In this section, we explore whether the effects of Estimize competition on the incumbents are increasing in competition intensity. Intuitively, FinTech competition is more intense when few analysts compete with many Estimize contributors. We create an indicator function returning 1 when the number of Estimize contributors covering firm *j* in quarter *t* exceeds the corresponding number of sell-side analysts, and 0 otherwise. We compute the variable *Estimize Firm* $Coverage_{jt}$ by first averaging the values this function takes in prior quarters, then converting this average to a 0/1 indicator variable based on its sample median.

Based on prior evidence that sell-side analysts compete not only at the firm level, but also at the industry level (Merkley et al. 2017), we suggest that sell-side analysts face more intense competition when they cover a firm in an industry with greater Estimize coverage. To measure Estimize industry coverage, we classify firms into 68 industries according to the Global Industry Classification Standard (GICS).[26] For each industry, we compute the total number of firms added to Estimize in 2012, scaled by the total number of firms in the industry as of 2012 (*Estimize Industry* $Coverage_{jt}$), and we convert this measure to an indicator based on its median breakpoint. Figure 2 reports the ten most heavily and ten least heavily covered industries. Industry coverage varies significantly, ranging from 67 percent to 83 percent in the ten most heavily covered industries and from 0 percent to 27 percent in the ten least heavily covered industries.

Finally, competition is likely to be more intense when Estimize quality, defined as unbiasedness or accuracy, is relatively higher. We create an indicator function returning 1 when Estimize consensus bias for firm *j* in quarter *t* is lower than the corresponding sell-side bias, and 0 otherwise. We calculate the variable *Estimize* $Unbiasedness_{jt}$ by first averaging the values taken by this function in all prior quarters, then converting this average to a 0/1 indicator based on its sample median. *Estimize* $Accuracy_{jt}$ is defined analogously.

To examine the relation between Estimize competition and sell-side forecast quality, we augment Equation (2) by interacting $Post \times Treated_t$ with our measures of Estimize competition intensity (*Estimize Firm Coverage, Estimize Industry Coverage, Estimize Unbiasedness,* and *Estimize Accuracy*).

Specifications 1 and 2 of Table 8 present the results when the outcome variables are *Bias* and *AbsFE,* respectively. In Specification 1, we find that sell-side consensus bias declines more when the Estimize consensus is relatively more unbiased and Estimize industry coverage is relatively greater, further corroborating and extending our Table 4 result of an average decline in sell-side bias. Specifically, when *Estimize Unbiasedness* and *Estimize Industry Coverage* increase from 0 to 1,

[23] Over our sample period, 84 percent of Estimize forecasts had a horizon of less than 120 days, and less than 5 percent had a forecast horizon of greater than one year.

[24] We do not examine the effect of Estimize on the accuracy of longer-term forecasts because we do not find that the accuracy of short-term forecasts is affected. However, in untabulated analysis, we also confirm that there is no significant change in *AbsFE* for longer-horizon forecasts.

[25] We omit forecasts with shorter horizons ($t = 2$, 3, or 4) because they are less biased, and because they may not be completely free from competitive pressure. In untabulated analysis, we find no evidence that the bias of these forecasts changes after the arrival of Estimize.

[26] The classification scheme, well accepted in the literature as an accurate representation of how brokerage firms organize equity research (e.g., Bhojraj, Lee, and Oler 2003; Boni and Womack 2006), includes ten sectors, 24 industry groups, 68 industries, and 154 subindustries. Our results are similar when we assign firms to 24 industry groups.

TABLE 7

Changes in Consensus Forecast Bias
Placebo Tests

	$Bias_5$ [1]	$Bias_6$ [2]	$Bias_7$ [3]	$Bias_8$ [4]	*Rec Level* [5]
Post × *Treated*	−4.67%	3.02%	−1.68%	−8.61%	4.78%
	(−0.31)	(0.17)	(−0.11)	(−0.62)	(0.24)
Log (*Size*)	9.01%**	19.46%***	19.59%***	20.74%***	−16.90%***
	(2.19)	(4.40)	(4.49)	(4.04)	(−2.88)
Book-to-Market	1.12%	2.99%	−0.06%	−4.13%	−2.09%
	(0.29)	(0.59)	(−0.01)	(−0.97)	(−0.34)
Guidance	−6.34%	−4.51%	−1.81%	−4.92%	6.44%
	(−1.25)	(−0.98)	(−0.41)	(−1.30)	(1.00)
Log (*I/B/E/S Coverage*)	−9.49%***	−10.66%**	−8.92%**	−8.92%**	14.75%***
	(−2.80)	(−2.50)	(−2.23)	(−2.45)	(3.18)
Log (*Turnover*)	−12.87%***	−9.21%*	−7.78%*	−9.06%**	12.17%***
	(−2.82)	(−1.70)	(−1.92)	(−2.12)	(2.94)
Log (*Volatility*)	9.11%	−1.62%	−2.25%	−0.54%	−26.75%***
	(1.44)	(−0.21)	(−0.30)	(−0.07)	(−2.93)
Return	16.64%***	20.63%***	22.83%***	16.14%***	−8.68%***
	(6.42)	(7.81)	(5.50)	(3.44)	(−2.83)
Log (*Forecast Age*)	8.01%***	6.54%***	7.55%***	8.50%***	2.66%
	(3.41)	(3.03)	(2.96)	(2.65)	(0.63)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Calendar Quarter FE	Yes	Yes	Yes	Yes	Yes
R^2	17.56%	22.93%	23.86%	21.25%	9.42%
Obs. (Firm-Quarters)	33,700	31,359	27,746	23,387	35,518

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 7 examines whether the findings of reduced bias in one-quarter-ahead earnings forecasts (Table 4) replicate for longer-horizon earnings forecasts and investment recommendations, research outputs that are free from Estimize competition. In Specifications 1–5, we repeat Specification 1 of Table 4 after replacing bias in one-quarter-ahead forecasts with bias in five- to eight-quarters-ahead consensus earnings forecasts, and the consensus recommendation level, respectively. We convert recommendation levels to numeric values as follows: 1 for Strong Buy, 2 for Buy, 3 for Hold, 4 for Sell/Underperform, and 5 for Strong Sell. Reported t-statistics are based on standard errors double-clustered by firm and calendar quarter.
Detailed variable definitions are available in Appendix A.

consensus bias declines by an extra 10 percent and 13 percent, respectively. As a reference, the coefficient on *Post* × *Treated* is 13 percent.

When the outcome variable is *AbsFE,* the coefficients on *Estimize Unbiasedness, Estimize Accuracy,* and *Estimize Industry Coverage* in Table 8 are negative and of similar magnitudes, but only one is significant at the 5 percent level (*Estimize Accuracy*), with the other two variables significant at a 10 percent level. However, adding these coefficients to *Post* × *Treated* yields −17.35 percent, statistically significant at the 1 percent level (untabulated), which means that when competitive pressures are sufficiently high, indicated by relatively more unbiased and more accurate Estimize consensus forecasts and relatively larger Estimize industry coverage, sell-side consensus accuracy indeed improves.[27]

Collectively, the evidence from this section suggests a robust and pervasive decline in consensus bias, and improvements in consensus accuracy limited to firms where analysts face higher competitive pressures.

Changes in Individual Forecast Quality

Changes in Individual Forecast Quality: Cross-Sectional Patterns

In this section, we exploit the richness of the individual analyst forecast data to shed light on the mechanisms through which competition operates. Specifically, we investigate whether differences in strategic bias, the opportunity to learn from Estimize

[27] We find similar results when *Estimize Firm Coverage, Estimize Industry Coverage, Estimize Unbiasedness,* and *Estimize Accuracy* are defined as continuous variables rather than indicator variables.

FIGURE 2
Estimize Industry Coverage: Most and Least Popular Industries

Panel A: Ten Industries with Highest Estimize Coverage

Rank	Sector	Industry	Estimize Industry Coverage
1	Industrials	Industrial Conglomerates	83.33%
2	Consumer Staples	Food & Staples Retailing	81.82%
3	Consumer Staples	Beverages	77.78%
4	Consumer Discretionary	Multiline Retail	75.00%
5	Consumer Discretionary	Specialty Retail	73.44%
6	Consumer Staples	Food Products	70.37%
7	Consumer Discretionary	Consumer Services	68.75%
8	Materials	Chemicals	68.00%
9	Industrials	Capital Goods	67.86%
10	Healthcare	Health Care Technology	66.67%

Panel B: Ten Industries with Lowest Estimize Coverage

Rank	Sector	Industry	Estimize Industry Coverage
1	Financials	Thrifts & Mortgage Finance	0.00%
2	Financials	Banks	5.63%
3	Financials	Insurance	6.98%
4	Real Estate	Equity REITs	8.62%
5	Utilities	Water Utilities	11.11%
6	Materials	Paper & Forest Products	14.29%
7	Telecom	Wireless Telecommunication Services	14.29%
8	Utilities	Gas Utilities	21.43%
9	Telecom	Diversified Telecommunication Services	23.08%
10	Healthcare	Biotechnology	26.67%

Figure 2 reports *Estimize Industry Coverage* for the ten most heavily covered industries by Estimize (Panel A) and the ten least heavily covered industries (Panel B). We classify firms into 68 industries using the GICS industry definitions. For each industry, we compute *Estimize Industry Coverage* as the number of firms added to Estimize in 2012, scaled by the total number of firms in the industry as of 2012.

forecasts, and competitive pressure among analysts lead to differences in how strongly analysts respond to Estimize's entry. We expect that strategic bias should be larger among analysts with an underwriting relationship with the firm (Michaely and Womack 1999) and among analysts who issue favorable investment recommendations (Chen and Matsumoto 2006). As a more direct test of the information learning hypothesis, we explore whether the extent to which forecast quality improves depends on whether the forecast is issued before or after an Estimize forecast for the same firm-quarter. Our final prediction is that sell-side analysts whose past forecasts exhibit lower quality face greater competitive pressures and improve the quality of their forecasts more than other analysts do. That is, the effect of increased competition is to make poor performance less persistent.

We define the outcome variable as analyst forecast quality relative to all other analysts following the same firm in the same quarter as follows:

$$RelFQ_{ijt} = \frac{\left(FQ_{ijt} - \overline{FQ}_{jt}\right)}{MaxFQ_{jt} - MinFQ_{jt}}, \tag{4}$$

where $\overline{FQ}_{jt}$, $MaxFQ_{jt}$, and $MinFQ_{jt}$ are the firm-quarter mean, maximum, and minimum of FQ_{ijt}, as defined earlier. By focusing on *RelFQ,* we control for time-varying firm-specific determinants of analyst bias and accuracy (see Clement [1999], Hong and Kubik [2003], and Horton et al. [2017] for a similar approach). We exclude firm-quarters with fewer than three analyst forecasts so that our measures of relative forecast quality are meaningful.

We estimate the following regression:

$$\begin{aligned} RelFQ_{ijt} = {} & \alpha + \beta_1 Post \times StratBias_{ijt-1} + \beta_2 Estimize\ Availability_{ijt} + \beta_3 Post \times LowRelFQ_{ijt-1} + \beta_4 StratBias_{ijt-1} \\ & + \beta_5 LowRelFQ_{ijt-1} + \beta_6 Controls_{ijt-1} + \varepsilon_{ijt}. \end{aligned} \tag{5}$$

TABLE 8

Change in Consensus Forecast Quality and the Intensity of Estimize Competition

	Bias [1]	***AbsFE*** [2]
Post × Treated (*PT*)	−13.38%*	4.04%
	(−1.85)	(0.55)
PT × Estimize Firm Coverage	−0.32%	2.70%
	(−0.07)	(0.61)
PT × Estimize Industry Coverage	−12.88%**	−8.19%*
	(−2.47)	(−1.80)
PT × Estimize Unbiasedness	−10.29%***	−6.33%*
	(−3.33)	(−1.73)
PT × Estimize Accuracy	1.51%	−6.88%**
	(0.47)	(−2.21)
Log (*Size*)	−11.89%***	−25.68%***
	(−3.69)	(−5.91)
Book-to-Market	3.77%**	6.83%***
	(2.20)	(2.73)
Guidance	−1.38%	−20.53%***
	(−0.31)	(−5.44)
Log (*I/B/E/S Coverage*)	−1.12%	0.30%
	(−0.44)	(0.13)
Log (*Turnover*)	−5.81%***	−4.21%**
	(−3.99)	(−2.05)
Log (*Volatility*)	7.31%	9.34%***
	(1.29)	(2.60)
Return	−1.51%	−6.98%***
	(−0.64)	(−3.73)
Log (*Forecast Age*)	4.00%*	−2.39%*
	(1.90)	(−1.77)
Firm Fixed Effects	Yes	Yes
Calendar Quarter FE	Yes	Yes
R^2	5.92%	17.49%
Obs. (Firm-Quarters)	36,511	36,511

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 8 examines how changes in consensus forecast quality vary with the intensity of Estimize competition. We examine four measures of competition intensity: *Estimize Firm Coverage, Estimize Industry Coverage, Estimize Unbiasedness,* and *Estimize Accuracy*. For *Estimize Firm Coverage,* we first define an indicator function returning 1 when the number of Estimize contributors in a firm-quarter exceeds the corresponding number of sell-side analysts, and 0 otherwise. We compute *Estimize Firm Coverage* by first averaging the values this function takes in prior quarters and then converting this average to a 0/1 indicator variable based on its sample median. We compute *Estimize Industry Coverage* as the number of firms in the industry added to Estimize in 2012 scaled by the total number of firms in the industry in 2012. We convert this measure to an indicator based on its median breakpoint. Industry classification is based on the GICS 68 industry grouping. To measure *Estimize Unbiasedness,* we first create an indicator variable returning 1 when Estimize consensus bias is lower than the corresponding sell-side bias in a firm-quarter, and 0 otherwise. We then calculate *Estimize Unbiasedness* by first averaging the values taken by this function in all prior quarters and then converting this average to a 0/1 indicator based on its sample median. *Estimize Accuracy* is defined analogously. We then repeat the analyses of Specifications 1 and 2 of Table 4 after interacting *Post* × *Treated* (*PT*) with each of these four indicator variables. Reported t-statistics are based on standard errors double-clustered by firm and calendar quarter.
Detailed variable definitions are available in Appendix A.

StratBias$_{ijt-1}$ is a vector of two indicator variables: *Underwriting,* equal to 1 if analyst *i* is employed by a brokerage firm that has served as a lead underwriter of firm *j*'s securities offering in the past three years, and *Rec Optimism,* equal to 1 if analyst *i*'s most recent recommendation for firm *j* is a Strong Buy. *Estimize Availability*$_{ijt}$ is an indicator equal to 1 if Estimize forecasts for firm *j* in quarter *t* are available before analyst *i* issues her forecast. ***LowRelFQ***$_{ijt-1}$ is a vector of two variables: *High Bias* and *High AbsFE,* indicators equal to 1 if analyst *i*'s quarterly forecast for firm *j* in quarter t–1 is in the top decile of *RelBias*$_{ijt-1}$ and *RelAbsFE*$_{ijt-1}$, respectively. *Controls* is a vector of broker, analyst, or forecast attributes, shown in prior work to influence forecast quality: *Broker Size, Firm Experience, General Experience, Firms Followed, Industries Followed, Forecast Age, Forecast Frequency,* and *Days Elapsed*. See detailed definitions in Appendix A. We convert all independent

TABLE 9

Changes in Relative Forecast Quality
Analyst Attributes

	RelBias **[1]**	***RelAbsFE*** **[2]**
Post × *Underwriting*	−3.32%**	−1.29%
	(−2.56)	(−1.21)
Post × *Rec Optimism*	−1.09%***	0.21%
	(−2.75)	(0.66)
Post × *Estimize Availability*	0.25%	−0.11%
	(0.42)	(−0.23)
Post × *High Bias*	−3.90%***	−2.33%***
	(−3.60)	(−2.63)
Post × *High AbsFE*	−1.42%	−2.34%***
	(−1.30)	(−2.61)
Underwriting	0.70%	0.94%
	(0.75)	(1.23)
Rec Optimism	0.38%	−0.39%*
	(1.41)	(−1.77)
High Bias	20.59%***	7.27%***
	(24.41)	(10.39)
High AbsFE	−5.25%***	6.61%***
	(−6.19)	(9.37)
Other Controls	Yes	Yes
R^2	1.02%	1.24%
Obs. (Analyst-Firm-Quarters)	246,808	246,808

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 9 reports the results from the following OLS regression:

$$RelFQ_{ijt} = \beta_1 Post \times StratBias_{ijt-1} + \beta_2 Post \times Estimize\ Availability_{ijt} + \beta_3 Post \times LowRelFQ_{ijt-1} + \beta_4 StratBias_{ijt-1} + \beta_5 LowRelFQ_{ijt-1} + \beta_6 Controls_{ijt-1+\varepsilon_{ijt}}$$

$RelFQ_{ijt}$ is either $RelBias_{ijt}$ or $RelAbsFE_{ijt}$, each calculated by subtracting the firm-quarter mean, $\overline{FQ}_{jt}$, from individual analyst forecast quality, FQ_{ijt}, and scaling the difference by the firm-quarter range, $MaxFQ_{ijt} - MinFQ_{ijt}$. *Post* is an indicator equal to 1 from 2013 to 2015, and 0 from 2009 to 2011. $StratBias_{ijt-1}$ is a vector of two indicator variables: *Underwriting,* equal to 1 if analyst *i* is employed by a brokerage firm that has served as a lead underwriter of firm *j*'s securities offering in the past three years, and *Rec Optimism,* equal to 1 if analyst *i's* most recent recommendation for firm *j* is a Strong Buy. $Estimize\ Availability_{ijt}$ is an indicator equal to 1 if Estimize forecasts for firm *j* in quarter *t* are available before analyst *i* issues her forecast. $LowRelFQ_{ijt-1}$ includes *High Bias* and *High AbsFE,* where *High Bias* is an indicator variable equal to 1 if analyst *i*'s forecast for firm *j* in quarter *t*–1 was in the top 10 percent of *RelBias,* and *High AbsFE* is defined analogously. Finally, the regression includes the following set of untabulated analyst-level controls: *Broker Size, Firm Experience, General Experience, Firms Followed, Industries Followed, Forecast Age, Forecast Frequency,* and *Days Elapsed.* We convert all independent variables to relative measures by subtracting the average value of each variable across all forecasts for the firm-quarter and scaling by the range of the variable during the firm-quarter. The sample includes the 772 treated firms over the 2009–2015 period, excluding 2012. The reported t-statistics, in parentheses, are based on standard errors clustered by firm × calendar quarter.
Detailed variable definitions are available in Appendix A.

variables to relative measures by subtracting the firm-quarter mean and scaling it by the difference between the firm-quarter maximum and minimum: $RelAttr_{ijt} = \frac{Attr_{ijt} - Attr_{jt}}{MaxAttr_{jt} - MinAttr_{jt}}$, similar to how we compute relative bias and accuracy. We conduct this analysis on a sample of treated firms (246,808 individual analyst-firm-quarters) to avoid the creation of triple interactions.[28]

Specifications 1 and 2 of Table 9 report the results for relative bias and relative accuracy, respectively. We observe a greater decline in relative bias for analysts who issue more optimistic recommendations and have an underwriting relation with the firm, corroborating the strategic bias mitigation hypothesis. Despite the inverse relation between bias and accuracy, we find no evidence that analysts with greater strategic bias begin to issue more accurate forecasts. One possible explanation is that the reduction in bias coincides with analysts losing valuable access to management (e.g., Lim 2001; Green, Jame, Markov, and Subasi 2014b); another is that the accuracy improvement due to the reduction in bias is too small to reliably measure.

[28] In unreported results, we repeat our analysis for matched control firms. We find that *Post* × *StratBias* and *Post* × *LowRelFQ* are statistically insignificant predictors of *RelBias* and *RelAbsFE*.

We find no evidence that analysts who can learn from available Estimize forecasts issue forecasts of higher quality than other analysts. This casts significant doubt on the view that Estimize forecasts for the same firm-quarter are a valuable source of information for sell-side analysts.

Finally, we find that persistence in poor research quality declines following the introduction of Estimize. In particular, relative to the pre-Estimize period, the most biased analysts experience improvements in both *RelBias* and *RelAbsFE,* and the most inaccurate analysts experience improvements in *RelAbsFE*. This finding is consistent with Estimize disciplining low-quality analysts into reducing bias and increasing effort.

Intensive Margin Test

The decline in consensus forecast bias reported in Table 4 is consistent with both individual analysts improving their research quality (intensive margin effect) and with lower-quality analysts reducing their coverage of treated firms in order to avoid direct competition form Estimize (extensive margin effect). In this section, we use individual analyst forecasts to verify that at least part of the decline in bias is attributable to an intensive margin effect. We estimate the following equation:

$$FQ_{ijt} = \beta_1 Post \times Treated_{jt} + Controls_{jt} + \psi_{ij} + \delta_t + \varepsilon_{jt}, \tag{6}$$

where FQ_{ijt} is either individual forecast bias or individual forecast accuracy. Individual forecast bias is computed as in Equation (1) after replacing the consensus forecast ($ConsF_{jt}$) with its constituents (F_{ijt}). $Controls_{jt}$ is a vector of control variables, as in Equation (2), except that *Forecast Age* is defined at the individual analyst level, and ψ_{ij} is an analyst-firm fixed effect. The inclusion of analyst-firm fixed effects ensures that identification comes from the change in forecast quality for the same analyst-firm pair. We weight each analyst-firm-quarter observation by the inverse of the number of analysts issuing forecasts for that firm-quarter because our estimate of treatment effect at the individual analyst level will be otherwise heavily influenced by firms with greater analyst coverage and, therefore, not comparable to our consensus-based estimate. The sample includes the same number of firm-quarters, but 464,039 analyst-firm-quarter observations.

Specification 1 of Table 10 tabulates the results for individual analyst forecast bias. The diff-in-diff estimate remains highly significant, indicating that the decline in consensus bias is at least partially attributable to individual analysts improving their forecast quality. The magnitude of this estimate (–18.61 percent) is roughly 25 percent smaller than the consensus-level estimate in Table 4 (–24.57 percent), which suggests that some of the reduction in consensus forecast bias may stem from lower-quality analysts dropping coverage of treated firms. Specification 2 indicates that there is no improvement in *AbsFE* at the individual analyst level, consistent with our findings of an absence in improvement in accuracy at the consensus level.

Extensive Margin Test

In this section, we explore whether the decline in consensus quality is related to lower-quality analysts reducing their coverage of treated firms by shifting their coverage toward control firms. We investigate this possibility by estimating the following model:

$$Cover\ Treated_{it} = \beta_1 LowRelAQ_{it-1} + \beta_2 Post_t LowRelAQ_{it-1} + \beta_3 Z_{it-1} + \psi_i + \delta_t + \varepsilon_{it}. \tag{7}$$

$Cover\ Treated_{it}$ is defined as the number of treated firms covered by analyst i in quarter t, scaled by the sum of treated and control firms covered by analyst i in quarter t. $LowRelAQ_{it-1}$, defined as $High\ Bias_{it-1}$ or $High\ AbsFE_{it-1}$, measures analyst quality relative to all other analysts in quarter t–1. *High Bias* is equal to 1 when analyst i's average relative bias across all firms she covers places her in the top decile of all analysts in that quarter, and 0 otherwise.[29] *High AbsFE* is computed analogously. *Post* is an indicator equal to 1 over the 2013–2015 period, and 0 during the 2009–2011 period. We include analyst-level controls (*Broker Size, Firms Followed, Industries Followed,* and *General Experience*), measured at the end of the prior quarter, in Z, and analyst and calendar quarter fixed effects. We require that an analyst issue forecasts for five or more firms in a quarter to more accurately measure relative quality. Standard errors are clustered by analyst and calendar quarter.

Specifications 1 and 2 of Table 11 report the results when $LowRelAQ_{it-1}$ is defined as $High\ Bias_{it-1}$ and $High\ AbsFE_{it-1}$, respectively. In Specification 1, the coefficient on $Post \times High\ Bias$ is negative and statistically significant, indicating that more biased analysts tilt their coverage away from firms on Estimize relative to other analysts. The typical analyst follows nine firms in our sample, so the –3.14 percent estimate is approximately equivalent to one in three highly biased analysts dropping one treated firm (or adding one control firm). In Specification 2, the coefficient on $Post \times High\ AbsFE$ is also negative, but not statistically significant. The combined evidence suggests that Estimize affects the behavior of biased analysts more than the behavior of inaccurate analysts, consistent with Estimize being more effective in exposing analyst bias rather than analyst inaccuracy.

---

[29] Analyst i's relative bias, $RelBias_{it-1}$, is calculated by averaging the relative forecast bias of her j forecasts in quarter t–1, $RelBias_{it-1}$ (see Equation (5)).

TABLE 10

Changes in Consensus Forecast Quality Intensive Margin Tests

	Bias **[1]**	***AbsFE*** **[2]**
Post × Treated	−18.61%***	−5.20%
	(−2.76)	(−0.74)
Log (Size)	−14.19%***	−28.72%***
	(−5.07)	(−7.89)
Book-to-Market	2.36%***	6.21%**
	(2.60)	(2.53)
Guidance	−3.19%	−22.08%***
	(−0.72)	(−5.33)
Log (I/B/E/S Coverage)	−0.53%	0.16%
	(−0.22)	(0.07)
Log (Turnover)	−4.54%***	−4.22%*
	(−3.96)	(−1.95)
Log (Volatility)	5.91%	6.31%
	(1.03)	(1.35)
Return	−1.76%	−8.47%***
	(−0.78)	(−4.44)
Log (Forecast Age)	4.04%*	−2.84%***
	(1.86)	(−3.04)
Analyst × Firm Fixed Effects	Yes	Yes
Calendar Quarter FE	Yes	Yes
R^2	5.39%	17.39%
Obs. (Analyst-Firm-Quarters)	464,039	464,039

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 10 reports the results from the following OLS regression:

$$FQ_{ijt} = \beta_1 Post \times Treated_{jt} + Controls_{jt} + \psi_{ij} + \delta_t + \varepsilon_{jt}.$$

The dependent variable, FQ_{ijt} is either individual forecast bias ($Bias_{ijt}$) or accuracy ($AbsFE_{ijt}$). All independent variables are identical to Table 4 except we replace firm fixed effects with analyst × firm fixed effects, and we measure *Forecast Age* at the forecast level rather than the consensus level. The difference-in-differences estimates reflect changes in the quality of coverage provided by the same analyst for the same firm (i.e., intensive margin effect). We weight each analyst-firm-quarter observation by the inverse of the number of analyst forecasts in the firm-quarter to make the individual analyst-level findings more comparable to the consensus-level findings. The t-statistics (in parentheses) are based on standard errors clustered by firm × calendar quarter.
Detailed variable definitions are available in Appendix A.

Additional Tests of Analyst Effort

Our evidence of a pervasive reduction in bias and a limited improvement in forecast accuracy suggests that the primary effect of Estimize is to discourage strategic bias rather than to encourage greater effort. As a further test of the increased effort hypothesis, we consider several aspects of research quality that can benefit from greater effort: forecast informativeness, timeliness, and frequency, and a direct measure of effort: length of questions asked by analysts at conference calls. Next, we briefly motivate each outcome variable, highlighting the trade-offs entailed.

As a price-based measure of quality, informativeness has the advantage of reflecting only effort directed at producing private information, valued the most by investors, and the disadvantage of reflecting perceptions of research quality. We compute *Informativeness* as the average two-day DGTW-adjusted absolute return around revision dates scaled by the average two-day DGTW-adjusted absolute return around non-revision dates.[30] In defining revision and non-revision days, we exclude days that occur within the three-day window around earnings announcements or earnings guidance.

[30] See Daniel, Grinblatt, Titman, and Wermers (1997) for a more detailed discussion of the construction of the DGTW benchmark portfolio.

TABLE 11

Changes in Analyst Coverage Decisions

	Cover Treated **[1]**	***Cover Treated*** **[3]**
High Bias	2.31%**	
	(2.13)	
Post × *High Bias*	−3.14%**	
	(−2.00)	
High AbsFE		0.75%
		(0.55)
Post × *High AbsFE*		−1.08%
		(−0.66)
Log (*Broker Size*)	0.76%*	0.75%
	(1.66)	(1.64)
Log (*Firms Followed*)	−2.14%***	−2.11%***
	(−2.69)	(−2.87)
Log (*Industries Followed*)	0.92%	0.92%
	(1.02)	(1.01)
Log (*General Experience*)	0.84%	0.86%
	(0.51)	(0.52)
Calendar Quarter FE	Yes	Yes
Analyst FE	Yes	Yes
R^2	71.93%	71.92%
Obs. (Analyst-Quarters)	17,573	17,573

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 11 tabulates the results from the estimation of the following OLS regression:

$$Cover\ Treated_{it} = \beta_1 LowRelFQ_{it-1} + \beta_2 Post_t \times LowRelFQ_{it-1} + \beta_3 Z_{it-1} + \psi_i + \delta_t + \varepsilon_{it}.$$

$Cover\ Treated_{it}$ is defined as the number of treated firms covered by analyst i in quarter t, scaled by the sum of treated and control firms. $LowRelFQ_{it-1}$ is either $High\ Bias_{it-1}$ or $High\ AbsFE_{it-1}$. $High\ Bias_{it-1}$ is an indicator equal to 1 if $RelBias_{it-1}$ is in the top decile of the distribution across all analysts for quarter t–1; $RelBias_{it-1}$ is constructed by averaging $RelBias_{ijt-1}$ (as defined in Table 9) over the number of firms forecasted by analyst i in quarter t–1. $High\ AbsFE_{it-1}$ is computed analogously. *Post* is an indicator equal to 1 over the 2013–2015 period, and 0 during the 2009–2011 period. Z includes *Broker Size, Firms Followed, Industries Followed,* and *General Experience*. The sample is limited to analysts who issue at least five forecasts in the previous quarter. Standard errors are clustered by analyst and calendar quarter.
Detailed variables definitions are available in Appendix A.

Forecast Frequency, defined as the average number of forecasts issued per analyst during the quarter, measures the flow of information from analysts to investors over the period. In contrast, consensus accuracy captures the quality of information at the end of the quarter.

Providing investors with an equally accurate forecast earlier, when there is less available information, creates more value for investors, but requires greater effort on behalf of the analyst. Consistent with this, Chiu, Lourie, Nekrasov, and Teoh (2021) show that analysts who more quickly revise their forecasts after earnings are announced are more likely to be named an All-Star and less likely to be demoted. We measure *Forecast Timeliness* as the number of days analysts take, on average, to revise their forecasts after quarter $t-1$ earnings are announced.

Finally, following Merkley et al. (2017), we measure analyst effort by the average length of analyst conference call questions (*Question Length*). The motivation is that text length is correlated with complexity (e.g., Lehavy, Li, and Merkley 2011; Loughran and McDonald 2014), which suggests that more difficult questions should be longer. Furthermore, Cohen, Lou, and Malloy (2020) find that analysts who are more likely to cater to management ask shorter questions, consistent with the intuition that shorter questions reflect lower effort.

We reestimate Equation (2) after replacing *ConsFQ* with *Informativeness, Forecast Frequency, Forecast Timeliness,* and *Question Length,* and we tabulate our findings in Specifications 1–4 of Table 12, respectively.[31] We find significant

[31] Both *Forecast Frequency* and *Forecast Timeliness* are mechanically related to *Forecast Age*. Thus, for these outcome variables, we exclude *Forecast Age* as a control.

TABLE 12

Changes in Analyst Effort

	Informativeness [1]	*Forecast Frequency* [2]	*Forecast Timeliness* [3]	*Question Length* [4]
Post × *Treated*	6.50%	−2.01%	−30.30%***	13.69%
	(0.51)	(−0.14)	(−3.22)	(1.44)
Log (*Size*)	6.87%*	−1.40%	−1.72%	−0.62%
	(1.68)	(−0.32)	(−0.33)	(−0.14)
Book-to-Market	1.05%	−4.28%	1.41%	1.98%
	(0.46)	(−1.34)	(0.64)	(0.80)
Guidance	−5.37%	73.81%***	33.81%***	−0.34%
	(−1.31)	(10.75)	(5.09)	(−0.30)
Log (*I/B/E/S Coverage*)	−6.16%*	4.49%**	−0.26%	2.03%
	(−1.85)	(2.25)	(−0.09)	(0.41)
Log (*Turnover*)	3.61%	2.34%	−3.35%	1.12%
	(1.17)	(0.71)	(−0.95)	(0.70)
Log (*Volatility*)	0.16%	−2.07%	11.05%	−3.99%
	(0.05)	(−0.46)	(1.31)	(−1.56)
Return	−1.94%	−2.87%	1.07%	5.61%
	(−0.71)	(−1.03)	(0.52)	(0.79)
Log (*Forecast Age*)	−4.99%**			−0.16%
	(−2.22)			(−0.07)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Calendar Quarter FE	Yes	Yes	Yes	Yes
R^2	4.17%	7.96%	8.23%	17.07%
Obs. (Firm-Quarters)	28,410	36,511	36,511	32,500

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively.
Table 12 repeats the analysis in Table 4 after replacing consensus forecast quality, $ConsFQ_{jt}$, with four measures of analyst effort: *Informativeness, Forecast Frequency, Forecast Timeliness,* and *Question Length*. *Informativeness* is the average two-day absolute abnormal return around revision dates scaled by the average two-day absolute abnormal return around non-revision dates, where both revision and non-revision dates exclude days that occur within the three-trading day window around earnings announcements or earnings guidance. *Forecast Frequency* is the total number of one-quarter-ahead forecasts issued for the firm-quarter scaled by the total number of analysts covering the firm. *Forecast Timeliness* is the average number of days between the previous earnings announcement and the forecast date. *Question Length* is the number of words spoken by analysts during the earnings conference call scaled by the number of questions asked. Reported t-statistics are based on standard errors double-clustered by firm and calendar quarter.
Detailed variable definitions are provided in Appendix A.

improvements in forecast timeliness, but no evidence of increased informativeness, forecast frequency, or question length.[32] This weak evidence, combined with our findings of limited improvements in accuracy, suggests that Estimize likely has some positive impact on analyst effort, but this impact is relatively modest, particularly when benchmarked against the strong effect of Estimize on strategic bias.

V. CONCLUSION

The last two decades have witnessed a sharp decline in information and communication costs, as well as the creation of new information sources, some of which are directly competing with, and potentially disrupting, traditional sources of investment research. Estimize, an open platform that crowdsources short-term quarterly earnings forecasts, presents a unique opportunity for examining the effects of FinTech competition on analysts.

Using a diff-in-diff approach, we find that firms added to Estimize experience a pervasive and substantial reduction in consensus bias and a limited increase in consensus accuracy. We find no evidence that long-term forecasts and investment recommendations become less biased, alleviating the concern that the documented reduction in bias is a response to broad

[32] In contemporaneous work, Banker, Khavis, and Park (2018) also find evidence that the introduction of Estimize is associated with an increase in analyst forecast timeliness.

economic forces. Furthermore, the improvements in forecast quality are stronger for firms where Estimize, through lower bias and better industry coverage, exerts stronger competitive pressure, and among individual analysts in greater need of disciplining: those affiliated with management and those issuing favorable recommendations. While Estimize entry and stock coverage are not exogenous, the richness and consistency of our findings lead us to conclude that FinTech-engendered competition improves sell-side research quality primarily by discouraging strategic bias.

Our study has limitations that present research opportunities. Specifically, our setting is less than ideal for testing whether FinTech competition induces greater analyst effort and/or increases the amount of information available to analysts because Estimize forecasts are not more accurate, lower bias and sizable timing advantage notwithstanding, and they are generally unavailable at the time of analyst forecasts. We suggest that Seeking Alpha may present better opportunities for information learning since Seeking Alpha's research articles often precede analyst forecasts in time, and appear to diminish the information content of analyst forecasts (Drake, Moon, Twedt, and Warren, 2021). We also acknowledge that a FinTech-induced decline in analyst bias may reduce the flow of information from managers to analysts, resulting in less accurate analyst research, but note that this adverse effect may be offset by the beneficial effects of increased effort/learning by analysts and increased public disclosure by managers.

REFERENCES

Altınkılıç, O., V. S. Balashov, and R. S. Hansen. 2019. Investment bank monitoring and bonding of security analysts' research. *Journal of Accounting and Economics* 67 (1): 98–119. https://doi.org/10.1016/j.jacceco.2018.08.016

Avery, C. N., J. A. Chevalier, and R. J. Zeckhauser. 2016. The "CAPS" prediction system and stock market returns. *Review of Finance* 20 (4): 1363–1381. https://doi.org/10.1093/rof/rfv043

Banker, R. D., J. Khavis, and H. U. Park. 2018. *Crowdsourced earnings forecasts: Implications for analyst forecast timing and market efficiency*. Working paper, Temple University, University of Buffalo, and University of Saskatchewan.

Barber, B. M., R. Lehavy, M. McNichols, and B. Trueman. 2006. Buys, holds, and sells: The distribution of investment banks' stock ratings and the implications for the profitability of analysts' recommendations. *Journal of Accounting and Economics* 41 (1-2): 87–117. https://doi.org/10.1016/j.jacceco.2005.10.001

Bartov, E., L. Faurel, and P. Mohanram. 2018. Can Twitter help predict firm-level earnings and stock returns? *The Accounting Review* 93 (3): 25–57. https://doi.org/10.2308/accr-51865

Becker, B., and T. Milbourn. 2011. How did increased competition affect credit ratings? *Journal of Financial Economics* 101 (3): 493–514. https://doi.org/10.1016/j.jfineco.2011.03.012

Bhojraj, S., C. M. C. Lee, and D. K. Oler. 2003. What's my line? A comparison of industry classification schemes for capital market research. *Journal of Accounting Research* 41 (5): 745–774. https://doi.org/10.1046/j.1475-679X.2003.00122.x

Boni, L., and K. L. Womack. 2006. Analysts, industries, and price momentum. *Journal of Financial and Quantitative Analysis* 41 (1): 85–109. https://doi.org/10.1017/S002210900000243X

Bradley, D., R. Jame, and J. Williams. 2022. Non-deal roadshows, informed trading, and analyst conflicts of interest. *Journal of Finance* 77 (1): 265–315. https://doi.org/10.1111/jofi.13089

Cen, L., C. Ornthanalai, and C. M. Schiller. 2017. *Navigating Wall Street: Career concerns and analyst transitions from sell-side to buy side*. Working paper, University of Toronto.

Chan, J., S. Lin, Y. Yu, and W. Zhao. 2018. Analysts' stock ownership and stock recommendations. *Journal of Accounting and Economics* 66 (2-3): 476–498. https://doi.org/10.1016/j.jacceco.2018.08.010

Chemmanur, T. J., M. B. Imerman, H. Rajaiya, and Q. Yu. 2020. Recent developments in the FinTech industry. *Journal of Financial Management, Markets and Institutions* 8 (1): 2040002. https://doi.org/10.1142/S2282717X20400022

Chen, H., P. De, Y. J. Hu, and B. H. Hwang. 2014. Wisdom of crowds: The value of stock opinions transmitted through social media. *Review of Financial Studies* 27 (5): 1367–1403. https://doi.org/10.1093/rfs/hhu001

Chen, Q., J. Francis, and W. Jiang. 2005. Investor learning about analyst predictive ability. *Journal of Accounting and Economics* 39 (1): 3–24. https://doi.org/10.1016/j.jacceco.2004.01.002

Chen, S., and D. A. Matsumoto. 2006. Favorable versus unfavorable recommendations: The impact of analyst access to management-provided information. *Journal of Accounting Research* 44 (4): 657–689. https://doi.org/10.1111/j.1475-679X.2006.00217.x

Chiu, P. C., B. Lourie, A. Nekrasov, and S. H. Teoh. 2021. Cater to thy client: Analyst responsiveness to institutional investor attention. *Management Science* 67 (12): 7455–7471. https://doi.org/10.1287/mnsc.2020.3836

Clement, M. B. 1999. Analyst forecast accuracy: Do ability, resources, and portfolio complexity matter? *Journal of Accounting and Economics* 27 (3): 285–303. https://doi.org/10.1016/S0165-4101(99)00013-0

Cohen, L., D. Lou, and C. J. Malloy. 2020. Casting conference calls. *Management Science* 66 (11): 5015–5039. https://doi.org/10.1287/mnsc.2019.3423

Coleman, B., K. Merkley, and J. Pacelli. 2021. *Human versus machine: A comparison of robo-analyst and traditional research analyst investment recommendations*. Working paper, Indiana University.

American Accounting Association

Costa, L. 2010. Facebook for finance. *Institutional Investor* 44: 54–93. Available at: https://www.institutionalinvestor.com/article/b150qd9q8d9k2r/facebook-for-finance

Daniel, K., M. Grinblatt, S. Titman, and R. Wermers. 1997. Measuring mutual fund performance with characteristic-based benchmarks. *Journal of Finance* 52 (3): 1035–1058. https://doi.org/10.1111/j.1540-6261.1997.tb02724.x

Dechow, P. M., and R. G. Sloan. 1997. Returns to contrarian investment strategies: Tests of naive expectations hypotheses. *Journal of Financial Economics* 43 (1): 3–27. https://doi.org/10.1016/S0304-405X(96)00887-2

De Franco, G., H. Lu, and F. P. Vasvari. 2007. Wealth transfer effects of analysts' misleading behavior. *Journal of Accounting Research* 45 (1): 71–110. https://doi.org/10.1111/j.1475-679X.2007.00228.x

Doherty, N. A., A. V. Kartasheva, and R. D. Phillips. 2012. Information effect of entry into credit ratings market: The case of insurers' ratings. *Journal of Financial Economics* 106 (2): 308–330. https://doi.org/10.1016/j.jfineco.2012.05.012

Drake, M., P. Joos, J. Pacelli, and B. Twedt. 2020. Analyst forecast bundling. *Management Science* 66 (9): 4024–4046. https://doi.org/10.1287/mnsc.2019.3339

Drake, M., J. Moon, B. Twedt, and J. Warren. 2021. *Social media analysts and sell-side analyst research*. Working paper, Brigham Young University, Georgia Institute of Technology, University of Oregon, and University of Connecticut.

Ernst & Young (EY). 2019. *Global fintech adoption index 2019*. Available at: https://www.ey.com/en_gl/ey-global-fintech-adoption-index

Fang, L., and A. Yasuda. 2009. The effectiveness of reputation as a disciplinary mechanism in sell-side research. *Review of Financial Studies* 22 (9): 3735–3777. https://doi.org/10.1093/rfs/hhn116

Farrell, M., T. C. Green, R. Jame, and S. Markov. 2022. The democratization of investment research and the informativeness of retail investor trading. *Journal of Financial Economics* 145 (2): 616–641. https://doi.org/10.1016/j.jfineco.2021.07.018

Gentzkow, M., and J. M. Shapiro. 2008. Competition and truth in the market for news. *Journal of Economic Perspectives* 22 (2): 133–154. https://doi.org/10.1257/jep.22.2.133

Gentzkow, M., E. L. Glaeser, and C. Goldin. 2006. The rise of the fourth estate: How newspapers became informative and why it mattered. In *Corruption and Reform: Lessons from America's Economic History*, edited by Glaeser, E. G., and C. Goldin, 187–230. Chicago, IL: University of Chicago Press.

Givoly, D., Y. Li, B. Lourie, and A. Nekrasov. 2019. Key performance indicators as supplements to earnings: Incremental informativeness, demand factors, measurement issues, and properties of their forecasts. *Review of Accounting Studies* 24 (4): 1147–1183. https://doi.org/10.1007/s11142-019-09514-y

Gomez, E., F. Heflin, J. Moon, and J. Warren. 2020. *Can financial analysis on social media help level the playing field among investors? Evidence from Seeking Alpha*. Working paper, Temple University, University of Georgia, and Georgia Institute of Technology.

Green, T. C., R. Jame, S. Markov, and M. Subasi. 2014a. Broker-hosted investor conferences. *Journal of Accounting and Economics* 58 (1): 142–166. https://doi.org/10.1016/j.jacceco.2014.06.005

Green, T. C., R. Jame, S. Markov, and M. Subasi. 2014b. Access to management and the informativeness of analyst research. *Journal of Financial Economics* 114 (2): 239–255. https://doi.org/10.1016/j.jfineco.2014.07.003

Grennan, J., and R. Michaely. 2020. FinTechs and the market for financial analysis. *Journal of Financial and Quantitative Analysis* 56 (6): 1877–1907. https://doi.org/10.1017/S0022109020000721

Hainmueller, J. 2012. Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis* 20 (1): 25–46. https://doi.org/10.1093/pan/mpr025

Hong, H., and M. Kacperczyk. 2010. Competition and bias. *Quarterly Journal of Economics* 125 (4): 1683–1725. https://doi.org/10.1162/qjec.2010.125.4.1683

Hong, H., and J. D. Kubik. 2003. Analyzing the analysts: Career concerns and biased earnings forecasts. *Journal of Finance* 58 (1): 313–351. https://doi.org/10.1111/1540-6261.00526

Horner, J. 2002. Reputation and competition. *American Economic Review* 92 (3): 644–663. https://doi.org/10.1257/00028280260136444

Horton, J., G. Serafeim, and S. Wu. 2017. Career concerns of banking analysts. *Journal of Accounting and Economics* 63 (2-3): 231–252. https://doi.org/10.1016/j.jacceco.2017.03.003

Iacus, S. M., G. King, and G. Porro. 2012. Casual inference without balance checking: Coarsened exact matching. *Political Analysis* 20 (1): 1–24. https://doi.org/10.1093/pan/mpr013

Irvine, P. 2004. Analysts' forecasts and brokerage-firm trading. *The Accounting Review* 79 (1): 125–149. https://doi.org/10.2308/accr.2004.79.1.125

Irvine, P., M. Lipson, and A. Puckett. 2007. Tipping. *Review of Financial Studies* 20 (3): 741–768. https://doi.org/10.1093/rfs/hhl027

Jackson, A. R. 2005. Trade generation, reputation, and sell-side analysts. *Journal of Finance* 60 (2): 673–717. https://doi.org/10.1111/j.1540-6261.2005.00743.x

Jame, R., R. Johnston, S. Markov, and M. C. Wolfe. 2016. The value of crowdsourced earnings forecasts. *Journal of Accounting Research* 54 (4): 1077–1110. https://doi.org/10.1111/1475-679X.12121

Kadan, O., L. Madureira, R. Wang, and T. Zach. 2009. Conflicts of interest and stock recommendations: The effects of the global settlement and related regulations. *Review of Financial Studies* 22 (10): 4189–4217. https://doi.org/10.1093/rfs/hhn109

Kothari, S. P., E. So, and R. Verdi. 2016. Analysts' forecasts and asset pricing: A survey. *Annual Review of Financial Economics* 8 (1): 197–219. https://doi.org/10.1146/annurev-financial-121415-032930

Lehavy, R., F. Li, and K. Merkley. 2011. The effect of annual report readability on analyst following and the properties of their earnings forecasts. *The Accounting Review* 86 (3): 1087–1115. https://doi.org/10.2308/accr.00000043

Lim, T. 2001. Rationality and analysts' forecast bias. *Journal of Finance* 56 (1): 369–385. https://doi.org/10.1111/0022-1082.00329

Lin, H. W., and M. F. McNichols. 1998. Underwriting relationships, analysts' earnings forecasts and investment recommendations. *Journal of Accounting and Economics* 25 (1): 101–127. https://doi.org/10.1016/S0165-4101(98)00016-0

Loughran, T., and B. McDonald. 2014. Measuring readability in financial disclosures. *Journal of Finance* 69 (4): 1643–1671. https://doi.org/10.1111/jofi.12162

Lourie, B. 2019. The revolving door of sell-side analysts. *The Accounting Review* 94 (1): 249–270. https://doi.org/10.2308/accr-52110

Ma, G., and S. Markov. 2017. The market's assessment of the probability of meeting or beating the consensus. *Contemporary Accounting Research* 34 (1): 314–342. https://doi.org/10.1111/1911-3846.12232

Malmendier, U., and D. Shanthikumar. 2007. Are small investors naive about incentives? *Journal of Financial Economics* 85 (2): 457–489. https://doi.org/10.1016/j.jfineco.2007.02.001

Mayew, W. J. 2008. Evidence of management discrimination among analysts during earnings conference calls. *Journal of Accounting Research* 46 (3): 627–659. https://doi.org/10.1111/j.1475-679X.2008.00285.x

Mehran, H., and R. M. Stulz. 2007. The economics of conflicts of interest in financial institutions. *Journal of Financial Economics* 85 (2): 267–296. https://doi.org/10.1016/j.jfineco.2006.11.001

Merkley, K., R. Michaely, and J. Pacelli. 2017. Does the scope of the sell-side analyst industry matter? An examination of bias, accuracy and information content of analyst reports. *Journal of Finance* 72 (3): 1285–1334. https://doi.org/10.1111/jofi.12485

Michaely, R., and K. L. Womack. 1999. Conflict of interest and the credibility of underwriter analyst recommendations. *Review of Financial Studies* 12 (4): 653–686. https://doi.org/10.1093/rfs/12.4.653

Mikhail, M. B., B. R. Walther, and R. H. Willis. 1999. Does forecast accuracy matter to security analysts? *The Accounting Review* 74 (2): 185–200. https://doi.org/10.2308/accr.1999.74.2.185

Ott, J., K. R. Subramanyam, and I. Zhang, 2020. *Pressure from the crowd: Crowdsourced earnings forecasts and earnings management*. Working paper, University of Minnesota, University of Southern California, and University of California, Riverside.

Park, C. W., and E. K. Stice. 2000. Analyst forecasting ability and the stock price reaction to forecast revisions. *Review of Accounting Studies* 5 (3): 259–272. https://doi.org/10.1023/A:1009668711298

Philippon, T. 2016. *The FinTech opportunity*. Working paper, New York University.

PricewaterhouseCoopers (PwC). 2017. *Redrawing the lines: FinTech's growing influence on financial services*. Available at: https://www.pwc.com/jg/en/publications/pwc-global-fintech-report-17.3.17-final.pdf

Schafhäutle, S. and D. Veenman. 2021. *Crowdsourced earnings expectations and the market reaction to street earnings surprises*. Working paper, University of Amsterdam.

Serafeim, G., J. Horton, and S. Wu. 2015. How banking analysts' biases benefit everyone except investors. *Harvard Business Review* (June 5). Available at: https://hbr.org/2015/06/how-banking-analysts-biases-benefit-everyone-except-investors

Stickel, S. E. 1992. Reputation and performance among security analysts. *Journal of Finance* 47 (5): 1811–1836. https://doi.org/10.1111/j.1540-6261.1992.tb04684.x

Sul, E. 2020. *Effects of FinTech and crowdsourced forecasting on firms: Evidence from Estimize*. Working paper, George Washington University.

The Economist. 2015. The fintech revolution. (May 9): 14.

Veenman, D., and P. Verwijmeren. 2018. Do investors fully unravel persistent pessimism in analysts' earnings forecasts? *The Accounting Review* 93 (3): 349–377. https://doi.org/10.2308/accr-51864

Xia, H. 2014. Can investor-paid credit rating agencies improve the information quality of issuer-paid rating agencies? *Journal of Financial Economics* 111 (2): 450–468. https://doi.org/10.1016/j.jfineco.2013.10.015

APPENDIX A

Description of Variables

Variable	Definition
Consensus-Level Attributes (Tables 1–8 and 12)	
$Bias_{jt} = \frac{Actual_{jt} - ConsF_{jt}}{Price_{jt-1}} * 100$	Where *Actual* is reported earnings, *ConsF* is the mean of all individual forecasts issued within 120 days of an earnings announcement, and *Price* is the stock price at the end of the prior year. We retain the most recent forecast for each analyst. We winsorize *Bias* at the 2.5th and 97.5th percentiles.
$Bias5_{jt}$	Five-quarters-ahead earnings forecast bias. In constructing the consensus forecast, we consider only individual forecasts issued between day 361 and day 480 relative to the earnings announcement.[a] $Bias6_{jt}$, $Bias7_{jt}$, and $Bias8_{jt}$ are computed analogously.
$AbsFE_{jt}$ (*Absolute Forecast Error*)	The absolute value of $Bias_{jt}$.
Coverage	The number of unique contributors or analysts providing forecasts in a firm-quarter.
$Forecast\ Age_{jt}$	The average age of the individual forecasts that make up the consensus forecast, where individual forecast age is computed as the number of calendar days between the issuance of the forecast and the subsequent earnings announcement date.
$Median\ Bias_{jt}$	An alternative measure of bias that replaces *ConsF* with the median of all individual forecasts issued within 120 days of an earnings announcement.
$Median\ AbsFE_{jt}$	The absolute value of *Median Bias*.
MBE_{jt} (*Meet-or-Beat Earnings*)	A dummy variable equal to 1 for firms that report earnings greater than or equal to the consensus, and 0 otherwise.
$Stat\_Fcst_{jt} = earn_{jt-4} + \theta_{j0} + \theta_{j1}(earn_{jt-1} - earn_{jt-5}$	Where $earn_{jt}$ is firm j's quarter t earnings and θ_{j0} and θ_{j1} are parameters of an autoregressive model in fourth difference estimated on the past 30 quarters of data.
$Statistical\ Bias_{jt}$	An alternative measure of bias that replaces *ConsF* with $Stat\_Fcst_{jt}$.
$Statistical\ AbsFE_{jt}$	The absolute value of *Statistical Bias*.
$Rec\ Level_{jt}$	The consensus recommendation level at the end of each quarter. Recommendations are converted to numeric values using the following scale: 1 for Strong Buy, 2 for Buy, 3 for Hold, 4 for Sell/Underperform, and 5 for Strong Sell.
$Informativeness_{jt}$	The average two-day DGTW-adjusted absolute return around revision dates scaled by the average two-day DGTW-adjusted absolute return around non-revision dates, where both revision and non-revision dates exclude days that occur within the three-trading day window around earnings announcements or earnings guidance.
$Forecast\ Frequency_{jt}$	The total number of one-quarter-ahead forecasts issued for the firm-quarter scaled by the total number of analysts covering the firm.
$Forecast\ Timeliness_{jt}$	The average number of days between the previous earnings announcement and the forecast date.
$Question\ Length_{jt}$	The number of words spoken by analysts during the earnings conference call scaled by the number of questions asked.
First-Year Treatment Definitions (Tables 4 and 6–12)	
Treated (*First-Year Treatment*)	An indicator variable equal to 1 for firms added to Estimize in 2012, and 0 for matched control firms.
Post	An indicator variable equal to 1 for the 2013–2015 period, and 0 in the 2009–2011 period.
Post_2013	An indicator equal to 1 for forecasts issued in 2013, and 0 otherwise. *Post_2014* and *Post_2015* are defined analogously.
Staggered Treatment Definitions (Table 5)	
Treated (*Staggered Treatment*)	An indicator variable equal to 1 for firms that are added to Estimize at any point prior to 2015 and have Estimize consensus forecasts in at least 25 percent of the quarters after being added to Estimize, and 0 for matched control firms.
Treated_2012	An indicator variable equal to 1 for firms that are added to Estimize in 2012, and 0 otherwise. *Treated_2013* and *Treated_2014* are defined analogously.
Post	An indicator variable equal to 1 for the eight quarters after the firm's addition to Estimize, and 0 in the eight quarters prior to being added to Estimize.
Firm Attributes (Tables 1–8, 10, and 12)	
$Size_{jt}$	Market capitalization, computed as share price times total shares outstanding as of the end of the year prior to the earnings announcement date.
$Book\text{-}to\text{-}Market_{jt}$	The book value of equity for the most recent fiscal year prior to the earnings announcement date, scaled by market capitalization on December 31 of the same fiscal year. We winsorize *Book-to-Market* at the 1st and 99th percentiles.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
$Guidance_{jt}$	A dummy variable equal to 1 if the firm issues earnings guidance during the quarter.
$I/B/E/S\ Coverage_{jt}$	The total number of sell-side analysts (in I/B/E/S) covering a firm in a year.
$Turnover_{jt}$	Average daily turnover, defined as share volume scaled by shares outstanding in the calendar year prior to the earnings announcement date. We winsorize $Turnover_{jt}$ at the 99th percentile.
$Volatility_{jt}$	The standard deviation of daily returns over the calendar year prior to the earnings announcement date. We winsorize *Volatility* at the 99th percentile.
$Return_{jt}$	The average daily market-adjusted return over the calendar year prior to the earnings announcement date.
Estimize Attributes (Table 8)	
$Estimize\ Firm\ Coverage_{jt}$	An indicator variable equal to 1 if the fraction of times that Estimize coverage is greater than I/B/E/S coverage (measured across all prior firm-quarters with available Estimize forecasts) is greater than the sample median.
$Estimize\ Industry\ Coverage_{jt}$	An indicator variable equal to 1 if the number of firms in the industry added to Estimize in 2012 scaled by the total number of firms in the industry in 2012 is greater than the sample median. Industry classification is based on the GICS 68 industry grouping.
$Estimize\ Unbiasedness_{jt}$	An indicator variable equal to 1 if the fraction of times the Estimize consensus is less biased than the sell-side consensus (measured across all prior firm-quarters with available Estimize forecasts) is greater than the sample median.
$Estimize\ Accuracy_{jt}$	An indicator variable equal to 1 if the fraction of times the Estimize consensus is more accurate than the sell-side consensus (measured across all prior firm-quarters with available Estimize forecasts) is greater than the sample median.
Relative Analyst-Firm-Level Attributes (Table 9)	
$RelBias_{ijt}$	A measure of relative bias, computed as the forecast bias of analyst i for firm j in quarter t ($Bias_{ijt}$) minus the average forecast bias for all analysts covering firm j in quarter t $\left(\overline{Bias_{jt}}\right)$, scaled by the range for the firm-quarter ($MaxBias_{jt} - MinBias_{jt}$).
$RelAbsFE_{ijt}$	A measure of relative accuracy, computed as the absolute forecast error of analyst i for firm j in quarter t ($AbsFE_{ijt}$) minus the average forecast error for all analysts covering firm j in quarter t $\left(\overline{AbsFE_{jt}}\right)$, scaled by the range for the firm-quarter ($MaxAbsFE_{jt} - MinAbsFE_{jt}$).
$High\ Bias_{ijt-1}$	An indicator variable equal to 1 if analyst i's forecast for firm j in quarter $t-1$ is in the top 10 percent of *RelBias*.
$High\ AbsFE_{ijt-1}$	An indicator variable equal to 1 if analyst i's forecast for firm j in quarter $t-1$ is in the top 10 percent of *RelAbsFE*.
$Underwriting_{ijt}$	An indicator variable equal to 1 if analyst i is employed by a brokerage that has served as a lead underwriter for firm j's securities offering in the past three years minus the average value of *Underwriting* across all analysts covering firm j in quarter t.
$Rec\ Optimism_{ijt}$	An indicator variable equal to 1 if analyst i's most recent recommendation for firm j is a Strong Buy minus the average value of *Rec Optimism* across all analysts covering firm j in quarter t.
$Estimize\ Availability_{ijt}$	An indicator variable equal to 1 if Estimize forecasts for firm j in quarter t are available before analyst i issues her forecast less the average value of *Estimize Availability* across all analysts covering firm j in quarter t.
$Broker\ Size_{ijt}$	The number of analysts employed by the brokerage house employing analyst i in quarter t minus the average of *Broker Size* for all analysts following firm j, scaled by the range for the firm-quarter (max − min).
$Firm\ Experience_{ijt}$	The number of years since analyst i issued her first forecast for firm j minus the average of *Firm Experience* for all analysts following firm j, scaled by the range for the firm-quarter (max − min).
$General\ Experience_{ijt}$	The number of years since analyst i issued her first forecast for any firm minus the average of *General Experience* for all analysts following firm j, scaled by the range for the firm-quarter (max − min).
$Firms\ Followed_{ijt}$	The number of firms analyst i follows in quarter t minus the average of *Firms Followed* for all analysts following firm j, scaled by the range for the firm-quarter (max − min).
$Industries\ Followed_{ijt}$	The number of two-digit SICs followed by analyst i in quarter t minus the average of *Industries Followed* for all analysts following firm j, scaled by the range for the firm-quarter (max − min).

(continued on next page)

APPENDIX A (continued)

Variable	Definition
Forecast Age$_{ijt}$	The number of days between analyst *i*'s forecast for firm *j* in quarter *t* and the earnings announcement day of firm *j* minus the average of *Forecast Age* for all analysts following firm *j*, scaled by the range for the firm-quarter (max − min).
Forecast Frequency$_{ijt}$	The number of forecasts issued by analyst *i* for firm *j* in quarter *t* minus the average of *Forecast Frequency* for all analysts following firm *j*, scaled by the range for the firm-quarter (max − min).
Days Elapsed$_{ijt}$	The number of days between analyst *i*'s forecast for firm *j* in quarter *t* and any previous forecast for firm *j* in quarter *t* made by any other analyst minus the average of *Days Elapsed* for all analysts following firm *j*, scaled by the range for the firm-quarter (max − min).
Analyst-Firm-Level Attributes (Table 10)	
Bias$_{ijt}$	Individual analyst forecast bias, defined as $\frac{Actual_{jt}-F_{ijt}}{Price_{jt-1}} * 100$, where *Actual* is reported earnings, *F* is the forecast of analyst *i* for firm *j* in quarter *t*, and *Price* is the stock price at the end of the prior year. We limit forecasts to those issued within 120 days of the earnings announcement and retain the most recent forecast for each analyst. We winsorize *Bias* at the 2.5th and 97.5th percentiles.
AbsFE$_{ijt}$	The absolute value of *Bias$_{ijt}$*.
Forecast Age$_{ijt}$	The number of days between analyst *i*'s forecast for firm *j* and the earnings announcement day of firm *j* in quarter *t*.
Analyst Attributes (Table 11)	
Cover Treated$_{it}$	The number of treated firms covered by the analyst in the current quarter, scaled by the sum of treated and control firms covered by the analyst in the current quarter.
RelBias$_{it}$	A measure of relative bias constructed by averaging *RelBias$_{ijt}$* over the number of firms forecasted by analyst *i* in quarter *t*.
RelAbsFE$_{it}$	A measure of relative accuracy constructed by averaging *RelAbsFE$_{ijt}$* over the number of firms forecasted by analyst *i* in quarter *t*.
High Bias$_{it-1}$	An indicator equal to 1 if *RelBias$_{it-1}$* is in the top decile of the distribution across all analysts for quarter $t-1$.
High AbsFE$_{it-1}$	An indicator equal to 1 if *RelAbsFE$_{it-1}$* is in the top decile of the distribution across all analysts for quarter $t-1$.
Broker Size$_{it}$	The number of analysts employed by analyst *i*'s brokerage house.
Firms Followed$_{it}$	The number of firms followed by analyst *i*.
Industries Followed$_{it}$	The number of two-digit SICs followed by analyst *i*.
General Experience$_{it}$	The number of years since analyst *i* issued her first forecast for any firm.

[a] More generally, for a *t*-quarters-ahead forecast, we consider only individual forecasts issued between days $(t-1) * 90 + 1$ and $(t-1) * 90 + 120$.